THE

ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

VOL. I. JULY, 1811. No. IV.

SELECTED PAPERS.

*Researches on the Oxymuriatic Acid, its Nature and Combinations; and on the Elements of the Muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution.**

By H. DAVY, Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.

From the London Philosophical Magazine, Vol. 36.

THE illustrious discoverer of the oxymuriatic acid considered it as muriatic acid freed from hydrogen,* and the common muriatic acid as a compound of hydrogen and oxymuriatic acid; and on this theory he denominated oxymuriatic acid dephlogisticated muriatic acid.

M. Berthollet,† a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxygen; and this idea for nearly twenty years has been almost universally adopted.

Dr. Henry, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained

* Mem. Acad. Stockholm for 1774, p. 94.

† Journal de Physique, 1785, p. 325.

that hydrogen was produced from it by electricity; and he attributed the phænomenon to water contained in the gas.*

In the Bakerian lecture for 1808, I have given an account of the action of potassium upon muriatic acid gas, by which more than one-third of its volume of hydrogen is produced; and I have stated, that muriatic acid can in no instance be procured from oxymuriatic acid, or from dry muriates, unless water or its elements be present.

In the second volume of the *Mémoires d'Arcueil*, MM. Gay Lussac and Thenard have detailed an extensive series of facts upon muriatic acid and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogen, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the Voltaic battery, effects no change in them; if it has been previously freed from hydrogen and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid, precisely the same as Libavius's liquor, is formed: it occurred to me, that if this substance is a combination of muriatic acid and oxyde of tin, oxyde of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor

* Philosophical Transactions for 1800, p. 191.

of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white color; some of it was heated, to ascertain if it contained oxyde of tin; but the whole volatilized, producing dense pungent fumes.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquor of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid; and I have stated that two compounds, one fluid and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphorous acid. It occurred to me, that if the acids of phosphorus really existed in these combinations, it would not be difficult to obtain them, and thus to gain proofs of the existence of oxygen in oxymuriatic acid.

I made a considerable quantity of the solid compound of oxymuriatic acid and phosphorus by combustion, and saturated it with ammonia, by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opake powder. Supposing that this substance was composed of the dry muriates and phosphates of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid, by a heat below redness, I conceived that, by igniting the product obtained, I should procure phosphoric acid; I therefore introduced some of the powder into a tube of green glass, and heated it to redness, out of the contact of air, by a spirit lamp; but found, to my great surprise, that it was not at all volatile nor decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance, that a substance composed principally of oxymuriatic acid and ammonia should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were by combustion, or the action of ignited hydrat of potash. When brought into the flame of a spirit lamp and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow color, and left a fixed acid having the properties of phosphoric acid. When acted on by red-hot hydrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hydrat of potash.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of MM. Gay Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thomson; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of color, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia; but as these compounds did not present the same uniform and interesting properties as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygen could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymuriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15

or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gases, and from five to six parts of nitrogen are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water; and MM. Gay Lussac and Thenard have stated that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapor, and a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though, in proportion as the gases were free from oxygen or water, this condensation diminished.

I mixed together sulphuretted hydrogen in a high degree of purity and oxymuriatic acid gas, both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapor was deposited; and the residual gas contained about $\frac{19}{20}$ of muriatic acid gas, and the remainder was inflammable.

MM. Gay Lussac and Thenard have proved by a copious collection of instances, that in the usual cases where oxygen is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed: now, as it is shown that oxymuriatic acid gas is converted into muriatic acid gas by combining with hydrogen, it is scarcely possible to avoid the conclusion, that the oxygen is derived from the decomposition of water,

and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygen in oxymuriatic acid gas.

MM. Gay Lussac and Thenard indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious that in this case they formed the same compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must lose its hydrogen, and the lead its oxygen; which of course would form water: these able chemists, indeed, from the conclusion of their memoir, seem aware that such an explanation may be given, for they say that the oxymuriatic acid *may be* considered as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find that, when mercury is made to act upon 1 in volume of muriatic acid gas, by Voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hydrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hydrogen is always from nine to eleven, the volume of the muriatic acid gas used being 20.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oxymuriatic and muriatic acids may be considered as an expression of facts; whilst the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful

and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

When oxymuriatic acid is acted upon by nearly an equal volume of hydrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogen, by the stronger affinity of the metal; and an oxymuriate, exactly similar to that formed by combustion, is produced.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxygen may be conceived to be supplied to the tin, and hydrogen to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxygen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hydrogen to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if M. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oxymuriatic acid with potassium: muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, is decompounded: the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogen are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygen should be admitted in

it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids, become ignited during their mutual agency; and such an effect might be expected in an operation so rapid, as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygen, exists in the general analogy of the compounds of oxymuriatic acid, and metals, to the common neutral salts: but this analogy, when strictly investigated, will be found to be very indistinct; and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water, and the metals of bases with hydrogen; and that, in the case of the action of oxymuriatic acid and metals, metal furnishes hydrogen to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogen evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogen with oxymuriatic acid, one quantity must always be separated. Hydrogen is disengaged from its oxymuriatic combination, by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogen is that which seems to adhere to oxymuriatic acid with the least force.

I have caused strong explosions from an electrical jar to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the Voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hydrogen, which I am inclined to attribute to the presence of

moisture in the apparatus employed; for I once obtained hydrogen from Libavius's liquor by a similar operation: but I have ascertained that this was owing to the decomposition of water adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of oxygen in oxymuriatic acid gas, a natural question arises concerning the nature of these compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygen than oxymuriatic acid, in the state in which it has been named, by Mr. Chenevix, hyperoxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygen or hydrogen, and form with each of them an acid compound; of which that with hydrogen has the strongest, and that with oxygen the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyperoxymuriates are decomposed by muriatic acid. Or, is hyperoxymuriatic acid, the basis of all this class of bodies, the most simple form of this species of matter?

The phenomena of the composition and decomposition of the hyperoxymuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxygenized, from hyperoxymuriate of potash by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a little oxymuriatic acid is generated, yet oxygen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange-colored fluid, produced by dissolving hyperoxymuriate of potash in sulphuric acid, affords only oxygen in great excess, and oxymuriatic acid.

When solutions of muriates or muriatic acid are electrized in the Voltaic circuit, oxymuriatic acid is evolved at the positive surface, and hydrogen at the negative surface. When a solution of oxymuriatic acid in water is electrized, oxymuriatic acid and oxygen appear* at the positive surface, and hydrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxygenized muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygen, or the basis of oxymuriatic acid.

If the facts respecting the hyperoxymuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oxymuriatic acid, potassium and oxygen. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxygen combined with the potassium, which we know has an intense affinity for oxygen, and which, from some experiments, I am inclined to believe, is capable of combining directly with more oxygen than exists in potash, than with the oxymuriatic acid, which, as far as is known, has no affinity for that substance.

It is generally supposed that a mixture of oxymuriatic acid and hyperoxymuriatic acid is disengaged when hyperoxymuriate of potash is decomposed by common muriatic acid† but I am satisfied from several trials, that the gas procured in this way, when not mixed with oxygen, unites to the same quantity of hydrogen,‡ as common oxymuriatic acid gas from manganese; and I find, by a careful examination, that the gas dis-

* The quantity of oxymuriatic acid in the aqueous solution is so small that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

† If hyperoxymuriate of potash be decomposed by nitric or sulphuric acid, it affords oxymuriatic acid and oxygen. If it be acted upon by muriatic acid, it affords a large quantity of oxymuriatic acid gas only. In this last case, the phenomenon seems merely to depend upon the decomposition of the muriatic acid gas, by the oxygen, loosely combined in the salt.

‡ This likewise appears from Mr. Cruikshank's experiments. See Nicholson's Journal, vol. v. 4to, p. 206.

engaged during the solution of platina, in a mixture of nitric and muriatic acids, which has been regarded as hyperoxymuriatic acid, but which I stated some years ago to possess the properties of oxymuriatic acid gas,* is actually that body, owing its peculiar color to a small quantity of nitromuriatic vapor suspended in it, and from which it is easily freed by washing.

Few substances, perhaps, have less claim to be considered as acid, than oxymuriatic acid. As yet we have no right to say that it has been decompounded; and as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxygen.

May it not in fact be a *peculiar* acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxygen or oxides, in their properties and powers of combination; but differing from them, in being for the most part decomposable by water. On this idea muriatic acid may be considered as having hydrogen for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymuriatic acid, may be regarded as analogous bodies. The combinations of oxymuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature

* The platina, I find by several experiments made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hydrogen of the muriatic acid attracts oxygen from the nitric acid. Oxymuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red color. *Nitrous* acid and muriatic acid produce no oxymuriatic acid gas. Platina, during its solution in perfectly formed aqua regia, gives only nitrous gas and nitrous vapor; and I find, that rather more oxymuriatic acid gas is produced, by heating together equal quantities of nitric acid of 1·45, and muriatic acid of 1·18, when they are not in contact with platina, than when exposed to that metal. The oxymuriatic acid gas produced from muriatic acid, by nitric acid, I find combines with about an equal volume of hydrogen by detonation.

of these substances. It is in many cases connected with false ideas of their nature and composition; and in a more advanced state of the inquiry, it will be necessary for the progress of science, that it should undergo material alterations.

It is extremely probable that there are many combinations of the oxymuriatic acid with inflammable bodies which have not been yet investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay Lussac and Thenard is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oxymuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phoshoric and muriatic acid; and the liquid I believe only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oxymuriatic acid.

It is evident, that whenever an oxymuriatic combination is decomposed by water, the oxide or acid or alkali or oxidated body formed must be in the same proportion as the muriatic acid gas, as the oxygen and hydrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements, in the different oxides, acids, and alkaline earths.

If, according to the ingenious idea of Mr. Dalton, hydrogen be considered as one in weight, in the proportion it exists in water, then oxygen will be nearly 7·5; and assuming that potash is composed of one proportion of oxygen, and one of potassium, then potash will be 48, and potassium* about 40·5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32·9, and muriatic acid gas, of course, by 33·9; and this estimation agrees with the specific gravity of oxymuriatic acid gas, and muriatic acid

* Supposing potash to contain nearly 15. 6 per cent. of oxygen.

gas. From my experiments, 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains; whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxygen with which the inflammable matter will combine.*

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian lecture, to show that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shown that

* I have stated in the last Bakerian lecture, that during the decomposition of the amalgam from ammonia, one in volume of hydrogen to two of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogen arises from the decomposition of water; then the oxygen that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogen, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of two of ammonia and one of hydrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phenomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, The metal of ammonia, in which hydrogen was so loosely combined as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxygen. The second, the metals of the alkalies and alkaline earths, in which the hydrogen was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hydrogen was more intimately combined; producing by union with oxygen, water not separable by any new attractions. The phenomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.

it is the simplest known form of the alkaline matter. This I think approaches to an experimentum crucis. Potash made by alcohol, and that has been heated to redness, appears to be a hydrate of potash, whilst the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent. of water to convert into a hydrate.

Amongst all the known combustible bodies, charcoal is the only one which does not combine directly with oxymuriatic acid gas; and yet there is reason for believing that this combination may be formed by the intermedium of hydrogen. I am inclined to consider the only substance produced by the action of oxymuriatic acid gas, and olefiant gas, as a ternary compound of these bodies; for they combine nearly in equal volumes; and I find that, by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor, and muriatic ether, as is probable from the ingenious experiments of M. Gehlen and M. Thenard, must be combinations of a similar kind, one probably with more hydrogen, and the other with more carbon.

One of the greatest problems in economical chemistry, is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oxymuriatic acid is very strong; but so likewise is their attraction for oxygen, and the affinity of their oxides for water. The affinities of oxymuriatic acid gas for hydrogen, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases, when it is intended to attempt to produce alkali. It is not difficult after these views to explain the decomposition of common salt, by aluminous or siliceous substances, which, as it has been long known, act only when they contain water. In these cases the sodium may be conceived to combine with the oxygen of the water and with the earth, to form a vitreous compound; and the oxymuriatic acid to unite with the hydrogen of the water, forming muriatic acid gas.

It is also easy, according to these new ideas, to explain the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity: the oxymuriatic acid is attracted by the lead, and the sodium combines with the oxygen of the litharge and with water to form hydrate of soda, which gradually attracts carbonic acid from the air.

As iron has a strong affinity for oxymuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings, and muriate of soda intensely heated: and in this way I succeeded in decomposing some of the salt: hydrogen came over: a little hydrate of soda was formed; and muriate of iron was produced.

It does not seem improbable, supposing the views that have been developed accurate, that by complex affinities, even potassium and sodium in their metallic form may be procured from their oxymuriatic combinations: for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society to some of the theoretical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phenomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form in this instance a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments on which these conclusions are founded, are, however,

uniform in their results: and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common reagents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? and that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Besides the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable that other compounds of like nature may be formed of the oxides, alkalies, and earths, with the oxymuriatic combinations, or of the oxymuriatic compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and, as it would seem at first view, contradictory facts. For if, as I have said, oxymuriatic acid gas be referred to the same class of bodies as oxygen gas, then, as oxygen is not an acid, but forms acids by combining with certain inflammable bodies, so oxymuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hydrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oxymuriates of phosphorus and tin.

Like oxygen, oxymuriatic acid is attracted by the positive surface in Voltaic combinations; and on the hypothesis of the connexion of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

I shall occupy the time of the society for a few minutes longer only, for the purpose of detailing a few observations connected with the Bakerian lectures, delivered in the two last years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquires have enabled me to correct or extend.

I have already mentioned that there are considerable differences in the results of experiments, made on the action of potassium, on sulphur and phosphorus, and their combinations with hydrogen, according to different circumstances of the process. I shall now refer to such of these circumstances as I have been able fully to investigate.

The able researches of Dr. Thomson have shown that sulphur, in its usual state, contains small quantities of acid matter; and though, in my first experiments, I conceived that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogen, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid; sulphuretted hydrogen likewise is soluble in muriatic acid: and this circumstance led me to underrate the quantity of sulphuretted hydrogen given off in experiments of this kind.*

* This circumstance has been pointed out by MM. Gay Lussac and Thenard, in a paper printed in the Journal de Physique for December, in which these gentlemen endeavour to show that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it pro-

In acting upon sulphuretted hydrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur or sulphuretted hydrogen was concerned, I have used muriatic acid saturated with sulphuretted hydrogen over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green-glass, or plate-glass lined with sulphur and filled with very pure nitrogen or hydrogen. In making potassium act upon sulphuretted hydrogen, I have employed the gas only in the quantities of from one to three cubical inches, and have made the combination in narrow curved tubes of green-glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When one grain of potassium, which would give by the action of water about one cubical inch and $\frac{1}{16}$ of hydrogen is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from $\frac{1}{14}$ to $\frac{1}{16}$ of a cubical inch of sulphuretted hydrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogen, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hydrogen.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hydrogen evolved by the action of the acid, is from $\frac{7}{10}$ to $\frac{9}{10}$; but if heat be applied to the combination, so as to drive

duces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries.

off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phenomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as three to one in weight; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1·1 cubical inches of sulphuretted hydrogen, all the hydrogen is set free, and a sulphuret of potassium containing one fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogen disengaged, and a compound of sulphuretted hydrogen and sulphuret of potash is formed, which gives sulphuretted hydrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments I am inclined to believe that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about ^{3} of a grain of phosphorus to form a phosphuret; which when acted upon by muriatic acid, produces from $\frac{8}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hydrogen.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogen, and sets free rather more than four cubical inches of hydrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combination were to be deduced from its union with potassium, in which it seems to form about one fourth the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuret-

ted hydrogen and sulphurous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan: that of sulphurous acid gas I find is 20967. Sulphuretted hydrogen, as I have shown, contains an equal volume of hydrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxygen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphurous acid from 100 of oxygen in volume; from which I am inclined to believe, that sulphurous acid consists of sulphur dissolved in an equal volume of oxygen; which would give the number as 13·7* nearly considering the acid gas as containing one proportion of sulphur, and two of oxygen; and these estimations do not differ from each other materially.

I have made several experiments on the combustion of phosphorus in oxygen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorbed in combustion about 34 of oxygen in weight: and considering phosphoric acid as composed of three portions of oxygen and one of phosphorus, the number representing phosphorus, will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphuret of potassium.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxygen and

* The estimation from the composition of sulphuretted hydrogen, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hydrogen at 35 grains the 100 cubical inches, which was not far from the mean between the estimations of Mr. Kirwan and Mr. Thenard. According to this last experiment, sulphuretted hydrogen is composed of one proportion of hydrogen, represented by 1, and one of sulphur represented by 13·4.

hydrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hydrogen, and probably contains that body. It is not unlikely, that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur, is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities in common phosphorus and sulphur, and with hydrogen give to them their variable properties.

The colors of different specimens of phosphorus, as well as of sulphur, differ considerably; the red color of phosphorus as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow, the Sicilian sulphur of an orange color, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish-green color. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygen in Sicilian sulphur, which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation, or to sulphuric acid itself.

Conceiving that, if definite proportions of oxygen and hydrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of sulphur with oxymuriatic acid gas, I employed five grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel

in which it had been in contact with warm water: in this case more than a half a cubical inch of oxygen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapor had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime: in this case, though Sicilian sulphur was used, no oxygen gas was evolved; and not a half a cubical inch of muriatic acid; the quantity was the same as in the last experiment; and it was found, that between 16 and 17 cubical inches of oxymuriatic acid gas disappeared; the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny orange color.

No oxygen was expelled during the combustion of phosphorus in oxymuriatic acid gas, nor could I ascertain that any muriatic acid had been formed; three grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas.

It would seem from these quantities, that the sulphuretted liquor formed by subliming sulphur in oxymuriatic acid gas, consists of one proportion of sulphur, represented by 13·5, and one of oxymuriatic gas represented by 32·9, and that the phosphoric sublimate must be composed of three portions of oxymuriatic gas, represented by 98·7 and one of phosphorus represented by 16·5.

Cases illustrating the Effects of Oil of Turpentine in expelling the Tape-worm.

By JOHN COAKLEY LETTSOM, M. D. and President of the Medical Society.

From the Philosophical Magazine, vol. xxxvi.

CASE I.

EARLY in September 1809, I was consulted by J. P. esq., about thirty-five years of age, on account of an uneasiness in the abdomen, with dyspepsia, which were supposed to originate from tænia, or tape-worm, as small portions of it had occasionally been evacuated by the rectum.

I prescribed a course of the male fern, with occasional cathartics, as recommended by Madame Nouflet. In this plan he persevered for the space of three months, in which period he discharged, at two different times, about eight yards of the tænia. In April 1810, he again applied to me, in consequence of labouring under his former complaints; adding that he imagined, from the long use of the plant I had recommended, his pains, and particularly the dyspepsia and general debility, had increased.

At this time I ordered the oleum terebinthinæ rectificatum, in a dose of nine drachms by weight, and after it a little honey to remove the heat and unpleasant taste it might occasion. In a week after taking the oil, he called upon me agreeably to my request, and gave me the following information: That as far as he could judge, in swallowing this medicine, it occasioned less heat than would have resulted from taking the same quantity of brandy, or other spirit; and that the taste and heat it produced were soon removed by the honey.

In about three hours after taking this dose, a laxative motion was produced, without any discharge of tænia: but soon afterwards, with the second stool, more than four yards of the worm were discharged, and also a quantity of matter, resembling, as the patient expressed it, the substance and skins of the tænia. On the surface of this evacuation he noticed the oil floating, together with the tænia and the substance mentioned. It produced little or no pain, and certainly much less than the purgative he had taken after the use of the male fern. The subsequent motions contained no tænia, nor any of the substance before noticed. He experienced no pain or heat in the urinary passages, though the urine continued to impart a terebinthinate smell for three or four successive days.

My patient has since remained in perfect health, enjoying a degree of comfort to which he had been a stranger for the preceding half year.

From this and other instances, I am induced to conclude that the best method of taking the oil is without any admixture; and that the dose of nine drachms occasions very little inconvenience: and farther, that this quantity, perhaps owing

to its quick purgative effect, excites no irritation in the urinary passages, although it imparts its peculiar smell to the urine.

I do not recollect that it has been heretofore observed, that the oil has been evacuated in its original state. It might hence be inferred, that it is most efficacious when exhibited uncombined, in which state it is not attended with any particular inconvenience.

It is well known that tænia may exist in a healthy state of the system; and that hence its presence cannot be accurately ascertained by any other circumstance than the actual discharge of portions of the worm itself. Sometimes, indeed, there is felt a heavy pain in the epigastrium, attended with dyspepsia and emaciation; but these are not pathognomonic symptoms, as they may arise from other causes.

CASE II.

By THOMAS HANCOCK, Physician to the Queensbury Dispensary.

I have used the ol. terebinth. in only one case of tænia. Jane Woodward, a poor woman, about 45 years of age, first applied to me at the London Electrical Dispensary, some months ago. She had been for more than seven years afflicted in a very distressing manner with this complaint, and was four times a patient in different hospitals, where by the use of active remedies, she obtained temporary relief from pain, and frequently voided large portions of the tænia per anum. So soon as she had recovered a little strength, by indulging her appetite, after the violent operation of purgatives in these hospitals, her abdomen began to increase considerably in size, and small detached portions of tænia, about an inch or more in length, apparently endowed with life, continued to pass at times through the rectum; so that she was prevented from earning her bread, by this very distressing circumstance. She had generally recourse to purgatives on these occasions, and their operation had regularly the effect of reducing the size of the abdomen; but her disease continued. I may also observe, that, after these courses of medicine, she had less of rumbling in the intestines, and felt less pain, than when she freely indulged her appetite; for then, to use her own expressions, "the worms appeared to gain strength," according to the increase of her own strength.

About two weeks after the application of electrical sparks to the abdomen, she discharged a portion of tænia, seven yards in length, without any appearance of head, which lived in cold water nearly three hours after its expulsion. Mr. Chamberlaine informs me he has known the tænia live nearly as long in water which was much above the temperature of the human body; a sufficient proof of the extraordinary tenacity of life in this animal.

Electricity was continued for some weeks longer; but as her pains also continued, and no more of the worm came away, my friend Mr. Chamberlaine kindly offered to try the effect of his electuary of the dolichos pruriens. The patient took this at first without, but afterwards with, the scobs stanni in large quantities, and for a considerable time: but though, as she asserted, these medicines, more than any she had ever taken, relieved her sufferings, they produced no discharge of tænia.

I now heard of the ol. tereb. having been administered in this complaint, and resolved to give it a fair trial, especially as my patient was herself very anxious to use any remedy that promised the slightest probability of success. I may premise, that her abdomen was enlarged as formerly, her stools slimy, and, in short, all her symptoms indicated that she had still large portions of tænia in her intestines. I ordered her at first small doses of this oil, viz. two drachms twice a day mixed with treacle to disguise its taste. This produced no other effect than an increase of pain and uneasiness, and particularly on going to stool, as if it irritated the rectum. The dose was now increased to half an ounce, at longer intervals. The first dose in this quantity, which she took without treacle, produced a little sickness and confusion of ideas, and afterwards operated as a purge. She complained of no uneasiness whatever in the urinary organs. After these doses, she passed such a quantity of slimy mucus, with such relief in all her painful symptoms, that she earnestly entreated I woud allow her to take a double dose. The quantity of an ounce, which she now took, always produced a great degree of giddiness, as if she was intoxicated, which came on shortly after taking it and con-

tinued for an hour or more, until the violent cathartic effect which followed, removed it.

Although this medicine was repeated, after this manner, every two or three days for a fortnight or more, by her own particular desire, there was no appearance of tænia in her stools. I could not, however, but observe, that the mucus which was discharged so abundantly by the operation of the ol. tereb. sometimes exhibited the appearance of white films, as if the substance of the worm had been broken down. She took the very large dose of an ounce and half two or three times, after the medicine began to lose its effect, with results similar to those I have described. In short, by her own account, violent purging was the only thing that relieved her; and all kinds of strengthening remedies, as well as nourishing diet, uniformly increased her sufferings, so that she abstained from food when her appetite craved it, in order to avoid the anticipated pain.

I have since heard that she went into the London hospital, and had again taken the ol. tereb.; for I strongly advised her to discontinue its use some time before she left the Finsbury Dispensary, having lost all hopes of its ultimately curing her.

CASE III.

By SAMUEL FOTHERGILL, M. D. F. M. S. Physician to the Western Dispensary.

A soldier aged about 40, applied to the Western Dispensary, the 28th of October, 1809. He stated that he had been subject to tape-worm during the last four years, previously to which he had served with the army in Egypt, and attributed the origin of his complaint to the badness of the water which he drank in that country. He is now a private in the Middlesex militia. He complains of gnawing pains in the abdomen, irregular appetite, debility, and anxiety. He is somewhat emaciated, and his complexion is rather sallow. Whilst with his regiment, he had occasionally taken, by order of the surgeon, a variety of worm medicines, and small pieces of tape-worm were passed at times; they even sometimes came away alive, without medicine having been taken, and without a stool.

I directed him to take pulv. scammon. cum calomel. ℈j.

every third morning. Two doses operated freely, but only a few very short pieces of tape-worm were brought away. I now directed him to take half an ounce of the oil of turpentine. He took it as ordered, November 9, in a little tea, sweetened with honey. In a quarter of an hour he was seized with retching, and in the course of the day passed four copious stools, in one of which was a tape-worm of several yards in length. The portion which the patient brought me I found measured four yards; he threw the smaller pieces away; but thought that altogether the length might be ten yards. The worm was dead, and had a livid appearance: the patient remarked that the pieces which formerly passed from him were of a whiter color and brighter aspect.

The dose of the medicine was increased to six drachms, and was repeated twice a week for the space of a month. During the first fortnight small pieces of worm continued to pass away, both after taking the medicine and at other times; but in the second fortnight the stools were natural, and contained no vestige of tænia. The remedy was consequently discontinued; and the man called some weeks afterwards to acquaint me that he had remained entirely free from all symptoms of his complaint, and had regained his strength and cheerfulness.

He was generally a little sick after taking the medicine, and for a day or two was affected with a severe pain in the back part of his head, but complained of no other unpleasant effects from its use.

CASE IV. and V.

By GEORGE BIRKBECK, M. D. F. M. S. Physician to the General Dispensary.

Dr. Birkbeck stated to the Society that he had administered the oil of turpentine, successfully, to two middle-aged females who had long been troubled with the tape-worm.

In the first case half an ounce was given: no unpleasant sensation occurred whilst it was swallowed, but considerable confusion of ideas and vertigo, with a slight degree of nausea, were soon produced. In a short time a discharge from the bowels took place; this was quickly followed by another, with which more than four yards of the worm were evacuated. The

patient, in consequence of the frequent spontaneous escape of small portions of tænia, and the expulsion of a larger quantity about twelve months before by an active purgative medicine, had an opportunity of comparing the ordinary appearance of the worm with that which it now presented. Instead of being whitish, smooth, full, and in motion, she represented it to be dark-colored, shrivelled, filmy, and lifeless. A second dose of the oil did not expel any more of the worm, nor, when he last saw her, about three months afterwards, had it again appeared. In that interval she had not been disturbed by any of the unpleasant feelings to which she before was subjected.

Considerable derangement of the general health and great pain in the pit of the stomach were produced by the tape-worm, in the second case in which the oil of turpentine was employed. Although one tea-spoonful only was introduced, sickness and acute pain followed: this dose was repeated several successive mornings, always with the same immediate effects; but occasionally it was succeeded by the expulsion of large portions of the worm. The worm was represented to have the appearance before noticed. The patient had sufficient resolution and confidence to continue for some time the use of the medicine, and at length became free, not only from any further appearance of tænia in the stools, but likewise from all those sensations which had so long denoted its presence in the intestines.

CASE VI.

By JAMES SANER, Surgeon, F. M. S.

A woman, about 40 years of age, came to me in May last, very much agitated, having just voided about six or eight feet of tape-worm. She told me, that pieces had come away for the last seven years whenever she took a dose of jalap, which she had done that morning. She never found any thing to relieve her so much as the jalap, though she had taken a great deal of medicine from respectable practitioners, and had also been under the care of a noted empiric for two years.

I thought this a good opportunity for trying the ol. tereb. rectificat. I therefore gave her one ounce with an equal quantity of syrup of saffron. In less than two hours she returned to

me with about eight feet of the worm, with the head attached. She was very much gratified by this, as she had been told to look for the small black head. The medicine did not produce any unpleasant sensation; merely a slight degree of nausea, a giddiness as if intoxicated, and a frequent desire to void urine, though without pain.

The day after, she complained of a feeling of emptiness in the stomach. I gave her the decoct. cinchonæ for a few days, which completely removed the sensation, and she has remained perfectly well ever since.

P. S. The woman informed me she used to eat raw meat formerly, as it seemed to ease her stomach more than any thing else; but since she voided the worm, she has had no craving for it.

CASE VII.

BY THE SAME.

Since communicating the above, I am sorry to say I have had a case of tænia, where the ol. tereb. rect. has not so completely answered my expectation.

Being very sanguine in my opinion of it, in consequence of my former success, I mentioned the case to a relation of mine, who informed me he knew a labouring mechanic (a Russian) who had voided large pieces of tape-worm for a number of years. He persuaded this man to visit me, and I gave him the same dose I had given my former patient. It brought away a very large quantity, but so very soft that I could not measure it. As I could not perceive any thing like the head of the worm, I advised him to repeat the dose in a few days, which he very readily complied with, as he had suffered very little from the first.

I gave him the same quantity as before (viz. ol. tereb. rect. et syr. croci āā℥j.) This produced violent retchings, tenesmus, strangury, and great pain in the back; the urine was also a little tinged with blood. The strangury and tenesmus continued nearly a week, and the patient was not able to work for several days after. As he had not voided any portion of worm with the last dose, I concluded that he was quite well, but re-

quested he would call on me again in about two months. He called last week, and I advised him to try his old remedy (a drachm of jalap), which had its usual effect, in bringing away a large quantity of the worm. I fear I shall not be able to induce him again to try the ol. tereb., from the severe symptoms which it produced when he last used it.

Extracts from an Essay towards an Inquiry how far the Effluvia from dead Animal Bodies, passing through the natural process of Putrefaction, are efficient in the production of Malignant Pestilential Fevers; and how far such Effluvia are capable "of exciting a Putrefactive Emotion in all other" living "Animal Substances exposed to their action?"

By C. CHISHOLM, M. D. F. R. S. &c. &c.

From the Edinburgh Medical and Surgical Journal, for October 1810.

THERE are grounds of belief, that even the concentration of the miasms of putrid animal substances, does not give rise to fever, and seldom, if ever, to disease of any description. The following facts certainly militate against a contrary conclusion.

1. In the neighbourhood of Bitton, in Gloucestershire, about a mile from Willsbridge, which was my residence for nearly four years, there is what is called, "a bone manufactory," in which animal bones, after the extraction of their medullary oil by boiling, are distilled, and yield the usual products, muriate of ammonia, and sulphate of soda. From this manufactory, a fœtor of the most offensive nauseating nature proceeds, and fills the atmosphere for nearly a mile around, diminishing in strength as it recedes from its source, and in proportion to its dilution or decomposition. The country is thickly inhabited, and near the manufacture itself is the village of Oldland, the population of which is very considerable; yet, in not one instance has this manufacture proved, in the smallest degree, injurious to health. I have frequently visited it with the most complete impunity. For several years the superintendent, Mr. Henderson, his wife, and family, lived in a house, having on one side, connected with it that in which the retorts are placed,

and on the other, that in which the bones are boiled; yet they had every appearance of health, and they assured me they enjoyed it. A more convenient house, on a neighbouring hill, becoming vacant, Mr. Henderson rented it for the accommodation of his family. Soon after they began to reside in it, they lost their health, and were, when I last saw them, much inclined to return to their old and stinking habitation. This exemption from disease in the manufactory of sal ammoniac, &c. has been noticed by Morveau and Chaptal. (Edin. Med. Journ. vol. ii. p. 295.)

2. Between Bristol and Hanham, on the banks of the Avon, is Conham, remarkable for nothing but its having been chosen for the site of an extensive manufactory for the conversion of the flesh of dead animals into a substance resembling spermaceti: a project which has been relinquished several years ago. This being also not very distant from Willsbridge, I made a good deal of inquiry into the result, as far as it affected the health of those immediately engaged in the process, and of the inhabitants of its thickly peopled neighbourhood. The foreman or superintendent, Richard Bolston, residing now at Jeffries-hill near Hanham, has been my principal informant: and his account was confirmed by that of Mr. Thomas Pearsall of Willsbridge, and other respectable persons. Bolston was two years employed constantly in this business; and during that time resided in the midst of dead animal bodies, horses, asses and dogs, many of which were left to pass through the natural process of putrefaction. He had three labourers under him, and he declares that neither himself nor any of these men suffered from a moment's sickness, or indeed experienced the smallest inconvenience. Their business was to cut up the carcasses, to strip the muscular flesh from the bones, and to dispose of it first in boxes, perforated for the admission of water, which were afterwards laid in pits filled with water. The entrails and every part not useful to them, were left to putrefy on the surface. The pits prepared for the animal matter thus disposed were seven feet deep, and four broad and long, and each calculated to contain the flesh of fifty horses, besides asses and dogs. An idea may therefore be formed of the immense volume of pu-

trid animal effluvia, enveloping continually the persons of Bolston and the labourers, by being informed that there were six of these pits, and consequently 300 carcasses of horses, and as many of asses and dogs, exhaling in greater or less abundance their offensive miasms. Notwithstanding this, Bolston declares, that although the stench was offensive in the highest degree, yet he and those with him sustained no injury: and to this the inhabitants of the country around bear ample and angry testimony both in relation to Bolston and themselves.

3. Another remarkable fact, well known where the manufactory of refined sugar is extensively carried on: butchers preserve the blood of the slaughtered animals in open tubs, kept in close small shut up houses, sometimes for several weeks, until the quantity required is completed, or until there is a demand from the sugar-bakers for it. It is then, in a putrid state, conveyed through the public streets in carts or drays to the sugar-houses, emitting the most offensive effluvia, and extremely annoying to all those who pass it. It is seldom immediately used by the sugar-bakers, but kept by them in casks in a putrid state, filling the air of the manufactory, and frequently of the vicinity, with its putrid miasms, or what Galen and his followers would call αἴτια λοιμῶν γενησόμενα, the seeds of pestilence. But what is the result to the workmen, or to the inhabitants of the surrounding houses? nothing inimical to health. This fact exists constantly in the city of Bristol, where in general the streets are extremely narrow, and the houses excessively crowded and ill ventilated: and yet the harmless nature of these exhalations may be daily verified: I speak from my own observation, and the experience of the most respectable sugar-bakers. In summer it is more remarkable than in winter.

4. Mr. Newman, surgeon in Stokes Croft, Bristol, a gentleman of great worth and professional skill, procured for me from his friend Mr. Bevington and his brother Mr. Newman of Bermondsey in Southwark, the following interesting particulars respecting the leather-dressing business.

"I have just received your letter of the 20th inst. (January 1810), making inquiry respecting putrid, contagious, and low fevers, as affecting the workmen employed by leather-dressers,

to which I can give you a pretty clear reply. Our men are generally healthy, and the most so of the labouring poor: many have been in our service and knowledge fifteen and twenty years, and I do not recollect one case of the kind occurring (in our establishment) in London. The first process in dressing is to put the skins into a pit of water to soften them, which is often used two or three times, that is, for two or three parcels, before it is changed, until the stench is intolerable. After this process the skins are struck out over a beam, and hung up, side by side, as close as possible, in a small room excluded from external air, which we term a stove: in this state they remain until they heat and slime, so that we can pull off the wool. The process of putrefaction is here so rapid as to disengage large quantities of volatile ammoniac, affecting the eyes of strangers with tears, and their noses with the most offensive smell. Our men always pull the skins in the stove in cold weather from preference, and are occupied in it a whole day at a time without injury." Another gentleman, a brother of Mr. Newman's, concerned in the leather-dressing trade, but not in the same house, in Bermondsey, informs him "that so far from our workmen being unhealthy or particularly subject to fevers, the reverse is the fact; the men employed look generally robust and healthy. In a concern in this line of business of fifty years' standing, in which fifty men are constantly employed, the men have been uniformly healthy; and in this a circumstance is deserving of notice, viz. the men who work upon the raw skins, from which there is a constant and profuse exhalation of putrid steams, and those employed at the lime and tan-pits, are equally healthy." Mr. Newman the writer of the above, says there are about sixty leather-dressers' and tanners' yards in Bermondsey, and in them about 700 men are constantly employed.

It may perhaps be objected to this account that the business of leather-dressers in other countries had been represented as extremely unhealthy. Hippocrates is supposed to have meant something of this kind as the cause, when he mentions the case of a person, Philiscus *residing near the wall*, who died on the sixth day of a malignant fever (Epidemic. Lib. i. s. 3.);

for anciently, and now indeed, offensive trades of this kind were carried on in the suburbs, παρα το τειχος of cities. This was the case at Rome, beyond the Tiber, and some of the Latin poets have exercised their wit in allusions to it. It is highly probable, however, that the real cause naturally existed in the spot itself set apart for the "sordidiores artes," and that what was attributed to them proceeded from the marshy nature of the soil. Certain it is, without recurring to this explanation, we cannot reconcile Mr. Bevington's and Mr. Newman's two respectable living witnesses, with the testimony of Ramazzini and Mercurialis, Martial and Juvenal, as quoted by him; and there is sufficient evidence that the Transtiberina Regio of Rome, and the Paduano (once, 17th century, male sanus, bestiis quam hominibus aptior) were proverbially unhealthy from their marshes, and that Bermondsey is not: See Ramazzini, De Morb. Artific. cap. 15.: and Annotat. in Lib. Lud. Cornelii, Veneti de Vit. Sobr. Commodis: and de Virg. Vestal. Valetud. tuend Dissertat.

5. I borrowed the following singular fact from the ingenuous and experienced Ramazzini. "In hac civitate (Modena), quæ pro suo ambitu satis populosa est, ideoque domos confertas habet ac prælatas, mos est ut tertio quoque anno in singulis domibus cloacæ expurgentur, quæ per vicos discurrunt. Cùm ergo domi meæ id opus fieret, contemplatus unum ex operariis istis in antro illo Charonæo magnâ anxietate ac sollicitudine opus suum peragentem, miseratus tam improbi laboris, ipsum interrogavi, cur tam sollicité laboraret, et non pacatius id ageret, ne ex nimio labore in multam lassitudinem incideret, tunc miser ex antro illo oculos attollens, meque intuitus: nemo, inquit, nisi expertus, imaginari potest, quanti constet, plus quàm quatuor horis in hoc loco morari, idem enim est cæcus fieri. Rursus ab eodem quæsivi, num in faucibus ardorem ullum persentiant, difficultatem aliquam respirandi patiantur, capitis dolore tententur, num odor ille nares percellat; nauseam pariat; nihil horum respondit ille, neque pars ulla in hoc opere mulctatur, præter oculos." This account was afterwards confirmed by his observing a number of these people reduced to blindness and beggary. "Oculis tamen solummodo, bellum

tam atrox indicunt fœtidæ exhalationes istæ, ac illos acutissimis spiculis sic feriunt, ut illis vitam, id est lumen, eripiant." Thus, as certain acrid substances seem exclusively to affect different and distinct parts of the body, as cantharides the bladder, the torpedo the nerves "sic halitus illi ex humanis fæcibus per varios corruptionis gradus trium annorum spatio, talem adsciscant naturam, ut oculos tantùm lacessant, cæteris vero partibus ignoscant." (De Morb. Artific. cap. 13.) This fact is no less important than curious, as it tends to show the inconsiderate conclusions of some eminent writers respecting the influence of the exhalations of privies on the health of men. Now whether the effect of these exhalations is asphyxia at Paris, according to Sauvages (Nos. Meth. i. 820), or amaurosis at Modena, according to Ramazzini, in either case there is ample proof that they cannot be productive of putrid or pestilential fevers.

6. The spontaneous extrication of putrid vapors from the sepulchral vaults of the cathedral church of Dijon, celebrated for having given occasion to the first experimental essays of oxygenants in the decomposition of putrid and contagious effluvia, may seem an exception. But this, in truth, can be considered only as a mephitis acting on those within its influence, in a greater or less degree, according to its concentration, and producing the usual effect of such effluvia, asphyxia. What M. de Morveau says of the appearance of a contagious fever in the neighbourhood, as connected with this mephitic vapor, is extremely vague and indeterminate. By sprinkling with a considerable quantity of the vinegar of the four thieves, "the odor of the putrid effluvia was merely masked for a moment, and soon reappeared with its former activity, *spreading to the neighbourhood, where the symptoms of a contagious fever began to appear.*" When the fumigation had been completed, he does not say that this contagious fever had been stopped or prevented: in fact he says nothing about it: he adverts only to the purification of a mass of air contaminated by these mephitic vapors. (See his Treatise on the means of purifying Infected Air, translated by Dr. Hall, p. 25–29. See also Sauvages,

Nos. Meth. tom. i. 819.) Ramazzini is by no means satisfactory on the subject, indulging in declamation, and exhibiting no proofs that "post magna prælia commissa, per insepulta cadavera, seu *per antiqua sepulchra incautè aperta*, diras pestilentias enatas, quæ ingentem populorum stragem ediderint." De Vespillonum Morbis.

A Case of Death, produced by Arsenic.

By JOHN YELLOLY, M. D. Physician to the London Hospital.

From the Edinburgh Medical and Surgical Journal, for 1809.

I AM induced, by the remarks made in the last number of the Edinburgh Medical and Surgical Journal, on the necessity of attention to individual cases of death by poison, to transmit to the editors the following history and dissection.

On the evening of the 21st of April last, I was requested to see the apprentice of a goldsmith, in Bell alley, Lombard street, who was reported to have taken some arsenic the night before. I went immediately, but, on my arrival, found that he had just expired. He had been attended by Mr. Unwin, apothecary, of Widegate, St. Bishopsgate, and from him, and the individuals of the family in which the young man lived, I obtained the following particulars.

He was about sixteen years of age, and informed Mr. Unwin, that, in consequence of a few words with his master, the day before, he had resolved to destroy himself; and that, at about ten o'clock the same evening, he drank off some gruel, in which he had dissolved, in a saucepan, over the fire, about a pennyweight of the white oxide of arsenic, of which there was generally some kept in the shop, for the purposes of the trade.*

On examining the saucepan, Mr. Unwin found in it the remains of the gruel, and about twenty-six grains of the white oxide of arsenic, in rough powder, though the lad told him,

* Arsenic, I am informed, is used in making a solder for inferior descriptions of gold work, called arsenic solder.

that he had only employed about a pennyweight, or twenty-four grains.*

His master's daughter observed him, in the course of the evening, throw something from a paper into a saucepan of gruel, and simmer it for a little time upon the fire. She and the maid servant saw him drink off the gruel; and the servant, having taken a tea spoonful of it, by his desire, was made very sick and uncomfortable the whole night. With regard to the quantity of arsenic which had been taken, it was supposed, from the diminution which was observed in a small packet of it, bought a short time before, that it must have been about half an ounce. But this is only conjecture. At any rate, the lad must have been much mistaken in the quantity which he imagined he had used, as there was more found in the remains of the gruel than he stated that he had put into it.

Soon after the gruel was taken, thus charged with arsenic, he was attacked with vomiting, and, in a short time subsequent to this, with purging; both of which continued with violence the whole of the night. He drank large quantities of both warm and cold water, but did not complain of any pain in the stomach or bowels, nor did it appear to the family that he suffered any. About four hours after taking the arsenic, he complained of a severe cramp in one of his legs. He was much distressed with a sensation of cold during the night, which made it necessary for him to have a great quantity of bedclothes.

Mr. Unwin saw him, for the first time, at half past ten on the morning after the arsenic was taken; his pulse was then not more than 40, and very languid. His lower extremities were cold, stiff, and without the power of motion; but, by the application of warmth, the stiffness was, in some measure, diminished, and the power of motion, in a small degree, restored. Mr. Unwin kept up the vomiting, by means of emetics and copious diluents; but the purging ceased about twelve

* As a proof that there was no mistake with regard to the nature of the powder found in the remains of the gruel, it may be proper to mention, that when made into a solution with carbonate of potash, it afforded a lively green precipitate on the addition of a solution of sulphate of copper.

o'clock, and the means employed to bring it on again were without effect.

At half past three, the extremities were cold, and he seemed unable to move them. He complained of great heat in his tongue; was very restless, and his pulse was rather more frequent than in the morning. The vomiting at this time had ceased. At about five, he appeared to be sinking fast; his pulse was as low as 30, and very languid; the upper, as well as the lower extremities, were cold and motionless; the restlessness was increased, and he expressed a great wish that sleep should be procured. He died a little before seven, which was nearly twenty-one hours after taking the arsenic. He retained the complete possession of his faculties till the very last, and never discovered any particular irritability of mind, except once or twice, when he could not get drink at the moment he wished for it. He was, at no time, in any degree convulsed, and showed no appearance of delirium. Mr. Unwin did not remark any tenderness of the abdomen, and never heard him express the existence of pain, either in the stomach or bowels. Indeed, when repeatedly questioned by him upon this point, the young man said that he felt no pain any where.

Mr. Unwin did not particularly attend to the sensibility of the extremities; but it was observed by him, that the changes of position which the young man was continually making, a few hours before his death, were effected principally by the trunk of his body, the extremities appearing inert.

The body was opened on the evening of Sunday, the 23d of April, about forty-nine hours after his death, by Mr. Morris, one of the house pupils of the London hospital, in the presence of Mr. Unwin and myself. It exhibited no particular signs of putridity; there was a slight lividness in the inside of the thighs, but nowhere else. On opening the abdomen, the stomach was found to be very large, but with no external mark of inflammation. The whole course of the small intestines, to within three inches of the caput coli, was very much inflamed, and the duodenum and jejunum were a good deal thickened. In some places, the external surface was of a florid red; but it was, for the most part, of a purple hue, and with occasional

patches of coagulable lymph. That part of the ilium which was free from inflammation (which was, as I have already said, a space of about three inches,) possessed its natural appearance, as did the whole of the cœcum, colon, and rectum. The colon and rectum, through their whole extent, were smaller than ordinary.

The inner membrane of the stomach, particularly at its lesser curvature, and in the neighbourhood of the pylorus, was very much inflamed, and there was in it frequent appearances of extravasated points of blood. In two or three circular spots, of the size of a shilling, the membrane was abraded; and, in one or two places, there was a circular thickening, (as if by an effusion of coagulable lymph,)* with some extravasated points of blood upon it. The stomach contained a considerable portion of a yellowish white fluid, with some castor oil floating upon it, which had been given in the course of the day of his death. In this fluid were found a grain of shot, a few small bits of carbonaceous matter, and a very small portion of a heavy, white, coarse powder, resembling the white oxide of arsenic.

The inflammation did not extend to the œsophagus. Portions of the small intestines were examined at several places, through their extent. The mucous membrane was every where found in a high state of inflammation, and with occasional appearances of extravasation in it. In the mucous membrane of the large intestines, the inflammation was much less, but it extended, in some degree, to the extremity of the rectum. There was no portion of arsenic or fæces found in any part of the alimentary canal.

The cohesion of the stomach and intestines seemed to be as great as in ordinary circumstances.

The absence of pain is a circumstance in which this case seems to differ from any which I have seen recorded; and I was therefore extremely particular in ascertaining this fact, not

* Dr. Baillie mentions having seen a thin layer of coagulable lymph, thrown out upon a portion of the inner surface of the stomach, in a case of death by arsenic.—Baillie's Morbid Anatomy, p. 138.

only from Mr. Unwin, but from every individual of the family who saw the young man previous to Mr. Unwin's being called in. The accounts were uniform and consistent; and there is the less reason to suppose any misconception upon this point, because the excruciating pain, which generally follows the exhibition of a large quantity of arsenic, could hardly, if it did exist, be concealed by the sufferer, or overlooked by the attendants.

It is rather unfortunate, that the precise quantity of the arsenic which was taken, in this case, is not known. The young man's own account, as above stated, was evidently far from the truth; while the diminution which was observed in the packet, may be considered to afford some grounds for believing that the quantity taken was very large.

To what extent the effects of arsenic are modified according to its quantity, is a circumstance concerning which there is little evidence; for, very few histories of persons poisoned by that substance have been detailed. That the sensorium is affected by its influence, is rendered probable, by the state of the pulse and of the extremities, in the case which I have now communicated. How far this affection of the sensorium is able to account for the absence of pain, can be at present only matter of speculation.

The effect of very long continued vomiting and purging, and of the use of a large quantity of diluents, would be to remove the poison which was swallowed. It may, therefore, be considered as probable, that the agency of such a substance may be sufficient to destroy life, though a very small portion, or none of it, should be retained.

The prominent effect of arsenic, taken into the stomach, is the production of inflammation; but this effect, like inflammation from any other cause, will doubtless continue after the cause producing it has been removed. There is, therefore, some reason to suppose, that the means generally employed in the treatment of such persons as have taken arsenic, whether they are directed to decompose, or to carry off this substance, are deficient, inasmuch as the inflammation actually produced by it is overlooked. The only remedy on which we can de-

pend for the cure of inflammation of the stomach or bowels, is copious bloodletting; and it may, therefore, be a question, how far this, in conjunction with the other means which may be thought necessary, should be early had recourse to, in cases where arsenic has been swallowed. Analogy seems to indicate its employment; but its particular fitness can only be determined by experience.

Finsbury Square, June 5th, 1809.

The following abstract of a case of Death from Arsenic is also deemed worthy of insertion. It may be consulted at large in the Transactions of a Society for the improvement of Medical and Chirurgical Knowledge. Vol. ii. p. 63.

Mary Hunt, on Thursday, the 19th of April, at twelve o'clock at night, took half an ounce of white arsenic, and immediately afterwards drank a quart of wine; about one o'clock she had so much pain in her stomach as to be obliged to call for assistance.

The symptoms were excruciating pain in the stomach, sickness, vomiting, excessive thirst, and a small tremulous pulse; these were followed by pain in the bowels, and several purging stools.

She drank brandy and water, wine and water, and several quarts of plain water to relieve the thirst, and ease the pain. Some hours after taking the arsenic she became easier, expressed a desire to be left alone, being inclined to sleep, and remained several hours in a dozing or comatose state, from which she did not recover, and died about one o'clock on Friday, thirteen hours after taking the arsenic.

Upon inspecting the body after death there were found the following appearances.

In the cavity of the abdomen there was an appearance of the effects of slight inflammation on the peritoneal coat of the small intestines.

The stomach contained a greenish fluid with a curdy substance in it, in all amounting to about twelve ounces. On the

internal surface of the great curvature near the cardia a portion of the villous coat about the size of a crown-piece was partly destroyed, and of a dark red color, with a regularly defined edge, and some of the arsenic adhering to different parts of its surface. The rest of the stomach was in a natural state. This appearance in the stomach was an effect produced by the arsenic.

Singular Case of Lameness.

Communicated in a letter from Dr. OSBORNE, of Cork.

From the Edinburgh Medical and Surgical Journal, for October, 1810.

A. B., aged twenty-three years, was, on the 3d of June, 1808, as well as ever in his life; but the next day, when marching with his regiment in Guernsey, for embarkation, was suddenly attacked with pain in his right side, so violent, that he could not stand; was shortly after brought to England, where the staff-physicians were of opinion that his liver was badly diseased. He was sent to his father in Ireland, where Drs. Pigot and Drew gave it as their opinion that he could not live. On this Dr. O. was sent for, who found him dreadfully reduced; his emaciation and debility excessively great; his tongue, from the manner of articulating, showing strong threatening of paralysis; pulse 120; tongue dry, with a dirty white crust upon it. The liver, on examination, found to be sensibly enlarged, and giving the feel as if pressing a tough dough or clay. His urine, in 24 hours, did not amount to three noggins, very high-colored, and depositing sediment between red and white. Bowels costive; affected only by medicine; appetite quite gone; his *right leg by three inches shorter than the left;* which, upon inspection, was found to be in consequence of the head of the thigh bone having slipt out of the acetabulum; and of this he could give no account, unless it happened when on board ship, whilst changing from hammock to hammock; but that he never had any sense of uneasiness or pain, either then or at any other time. From his emaciated state, fever, diminished urine, impediment of his tongue in articulation, Dr. O. conceived he could not live, and that he might be cut off sud-

denly; but ordered the squil-soda and hydrargyrus pill; that he should drink double soda-water, or plain water; no wine; and that his diet should be light and nourishing. In about six weeks, he came to town to have a consultation with Dr. Longfield, when he had the appearance of hale health in every respect, excepting the dislocation, which much surprised Dr. L.; who was so particular in his examination, that he said "the head of the thigh-bone is pushed directly upwards, and here is the protuberance formed by it distinctly to be felt." As the pills that had been ordered had set his kidneys active, by the discharging of large quantities of urine, they were now dispensed with, and he was put on a course of bark. This he continued for the course of a month, and, at the end of the time, returned to Cork, with both extremities of the same length. He was stripped, and the head of the femur was found to be perfectly in the acetabulum.

This is a most extraordinary case, and can be only accounted for from an excessive degree of relaxation of the rotund and capsular ligaments. For, if the bone had been forced out accidentally, a rupture of the ligaments must have been the consequence, and of course the bone would remain out; but the contrary must have been the case; for as the constitution recovered its natural powers, so the ligaments recovered theirs, and, by gradual contraction, brought about the reduction.

On reducing Dislocations.

By BRUDFORD WILMER.

From the Medical and Physical Journal, for December 1810.

THE following remarks on dislocation of the limbs have occurred, on reading a very interesting paper on the subject, published in the Medical and Physical Journal in April, 1808. After offering some very sensible strictures on the present mode of reducing luxations, in counteracting, by violent extension, the resistance of the muscles, (as he emphatically terms it) *by main force of surgery;* Mr. Carwardine proposes

to apply a regular and long continued extension, to exhaust their action. This he illustrates by the following case.

A strong muscular man dislocated his shoulder. Attempts to reduce it were ineffectually made in the country where the accident happened; and after an interval of fourteen days, he applied to an hospital which Mr. Carwardine was then attending.

One of the senior surgeons proceeded to the reduction of the joint. After repeated and violent attempts, which proved ineffectual, it was proposed to the man to remain in the hospital till the following day, and in the interim, that he should lose blood, take a cathartic, and keep a relaxing poultice on the joint. His business, however, not allowing of a longer continuance in the hospital, he was about to quit it, when Mr. Carwardine persuaded him to go to his lodgings. Having provided the necessary apparatus, the patient was placed on a stool, the scapula and trunk firmly fixed, and a set of pulleys accurately adjusted and secured to the condyles of the humerus, for the purpose of making extension. The cord was now tightened with a force just sufficient to counteract the gravity of the limb, and prevent its falling to his side. Mr. W—— with one hand held the wrist, to preserve the forearm bent, and with the other held the cord to keep it from relaxing, but without tightening it. This extension was continued for ten minutes, without producing any manifest effect. As the extension gave very little pain, it was slightly increased. In five minutes more the man grew very restless, would not allow that he then suffered pain, but said, *he felt very faint*, begged that Mr. C. would desist, and asked for water which was refused, under the idea, that if he fainted the muscles would be completely relaxed. At this moment he changed countenance, and the deltoid muscle was perceived to be tremulous. The cord was tightened a little: the arm followed it; the cord being now relaxed, the head of the bone slipped into its place, and the reduction was accomplished without any perceptible sound.

Mr. Carwardine attributes the success of this experiment to the long continued extension, having exhausted the action of those muscles connected with the dislocated limb; but from

cases which have occurred in my practice, it seems more probable it was the effect of that disposition to faint, which the man complained of towards the latter end of the experiment. In a state approaching to syncope, the circulation becomes weak, the action of all the voluntary muscles is nearly suspended, and if at that favourable moment, an extension could be made in cases of dislocated bones, I believe it would be generally successful. Surgeons who have seen much of practice must have remarked, when they have been called to patients who have suffered these accidents in a state of intoxication, with how much comparative ease, the reduction has been accomplished. In this state all the powers of the body are infeebled, and the muscular action, for a time, almost destroyed.*

Previous to the action of an emetic, the face becomes pale, and faintness is generally induced. At this critical time, if an extension is made, the reduction of luxated bones is accomplished with a facility, that will surprise those who have not seen the experiment. When cases have occurred in my practice, which have resisted the common methods of reduction, I have ordered an antimonial emetic, and making an extension when the patient first complains of faintness or sickness, have always succeeded, and with very little exertion of force.

Mr. Lloyd, aged about forty, a very strong and muscular man, dislocated his shoulder by a fall down a precipice; as it was late at night, he got no assistance till the following day. At seven o'clock in the morning, an eminent surgeon of this city was called to him. The trunk of the body was firmly fixed to a wall, and extension made from the lower part of the humerus, with the forearm bent, without success. Pulleys were then applied, and much force used, but the head of the bone was immovably fixed. Extension was then made in every direction in vain. These experiments were reiterated during the space of an hour, till the patient and the surgeon were both much exhausted. At eight o'clock, I was called into consultation, and proposed, that five grains of tartarized antimony

* Mr. Chesher has published a case, in which the reduction of a luxated Os Humeri was facilitated by the exhibition of emetic tartar. Med. Journal. vol. 8, p. 189.

should be given, every thing being prepared for extension; in five minutes the patient said he felt ill, in three more that he thought he should be sick. By a moderate degree of force applied to the lower part of the bone, in the same direction as that first applied, in a few seconds the reduction was accomplished with very great ease.

On the fourth of September last, a stout muscular young man of this city was thrown out of an open carriage and dislocated his arm. The next day I was desired to assist his surgeon, Mr. Kann, in the reduction. He had a very bad night, without sleep, in constant pain, was feverish, and the parts over the joint, tense and swelled.

A very forcible extension was made in the usual way, but the head of the bone was firmly fixt under the pectoral muscle. The patient was in a state, in every respect, the most improper, for the application of much force; I, therefore, proposed to Mr. Kann, to give him five grains of tartarized antimony. In seven or eight minutes he complained of being faint. I now remarked to Mr. Kann, the alteration which had taken place in the state of the parts about the joint. The muscles, before hard and tense, had become soft and flabby. Extension being then made in the same direction and manner as before, the reduction was immediately accomplished.

It was the opinion of Dr. Hunter, that in complete luxations of the os humeri, the capsular ligament was always ruptured, and that the nature of the laceration would sometimes render the reduction impracticable. And Mr. Thompson* has supposed, that the head of the bone might be tightly embraced by the tendons of the subscapularis and teres minor muscles, passing obliquely over it, and thus form an impediment to reduction. I believe it is now generally admitted, when these difficulties occur, that they are occasioned by the muscular power counteracting, with effect the extending force applied to them. Mr. Pott, aware of this circumstance, has given precise directions (by the position of the limb) to place the muscles, sur-

* Medical Observations and Inquiries, vol. 2, p. 355. Potts's Works, fol. edit. p. 693.

rounding and connected with luxated bone, in that state where they are likely to give the least resistance. But it will be found impossible to place a limb in any position, in which some of the muscles will not be more or less elongated. If the flexor muscles are relaxed, the extensors are, in the same proportion, stretched. Hence, it appears to be a great desideratum to affect the system in such a manner that *all the muscles* connected with the luxated bone may for a short time be deprived of their powers of making resistance.

In cases where great force and violent extension have produced no effects upon the dislocation, it has been the practice of surgeons to reduce the strength of the patient by large bleeding, baths, cathartics, and to relax the parts by fomentations, poultices, &c. That these sometimes fail is too well known to be insisted upon. The exhibition of an antimonial emetic in a few minutes reduces the muscles into a state of inaction; the other plan will not have the same effect in as many days. From so much evacuation constitutional effects may arise. From the former plan the patient's general health may be improved, and his recovery will be very speedy. He should go to his bed after the operation of the emetic, when a perspiration ensues, which will remove that feverish action of the system, the general accompaniment of accidents of this nature. The muscles also at the time of the reduction are so much relaxed that no pain attends the operation.

Extract of a letter to N. Bruce, Esq. Surgeon to the Forces, concerning the Endemial Fever of Sicily. Originally communicated in September, 1809, *to Dr. Franklin, Inspector of Hospitals.*

By ALEXANDER BOYLE, Surgeon, 62d Regiment of Foot.

As I had observed, some time before the return of the troops from Ischia, a disposition in the fever, prevailing in Melazzo, to assume the form of synocha; and aware of the ravages *fever* had made among the corps quartered in that district, for some seasons previously to my visiting that part of Sicily, I resolved to watch its progress.

In the first cases which offered themselves to my notice and which occurred about the beginning of July last, there appeared little which could, in a particular manner, demand my attention, beyond what is generally observed in those fevers to which we usually give the name of synocha.

At this period (from which we may date the commencement of the disease this season), it was ushered in with a cold stage; there was great languor and lassitude; hot dry skin; frequent and full pulse, and great thirst. The tongue was white and moist; the countenance flushed; and the eyes more or less diffused with blood. Head-ache, giddiness, and lassitude were, however, the symptoms of which they chiefly complained.

Until the middle of July, however, cases of fever were very rare; and such as did occur were easily overcome by cold affusion, a free use of saline purgatives, and plentiful draughts of iced lemonade. Bloodletting, at this period, was not necessary. In two cases, which occurred in children, the disease terminated by hemorrhagy from the nose; and in both, it was followed by a pustular eruption about the head and face. In a healthy girl of seventeen years of age, it followed the sudden interruption of the menstrual discharge, and ceased soon after that reappeared.

Though I kept no regular meteorological tables, I remarked that the weather, from about the 1st of July, became extremely oppressive, and that no rain had fallen for several weeks; the thermometer usually ranging from 84° to 88°, and a Sirocco wind generally prevailing. But the facility with which the disease was in these cases overcome, as yet led me to expect nothing extremely alarming.

On the 15th, however, two men came from the telegraph post at Archi, near Melazzo, which had always been deemed an unhealthy spot. Their cases, of course, became extremely interesting. From the great mortality at this place, it was never visited without apprehension. It became, therefore, a matter of importance to ascertain how far such apprehension was well founded. I went there, accompanied by Dr. Calvert, physician to the forces; and we were aware that the situation was reputed unhealthy, from exposure to what, in Sicily, is

generally called *mal' aria.* If by this is understood nothing more than *marsh miasma*, it may be said, that, though in autumn it may become the parent of many intermittents, at the time of our visit, there appeared no probable source from which it could be produced; and except one small inconsiderable stream, there was not a drop of water within two or three miles of the place. We ascertained, however, that the daily duty of the three men at that telegraph, exposed them alternately to the action of the solar heat, as long as it was possible to communicate with the adjoining posts. This discovery was at least sufficient to satisfy us, as to the object of our inquiry; and, without the assistance of other agents it will be allowed to be adequate to the production of the appearances discovered on dissection, in two cases I am about to relate.

The first whom I saw, observing the situation of his two companions, hastened to Melazzo. He rode about five miles. His pulse was weak and trembling, and about 110 in the minute. The muscles of his face were relaxed, and his countenance was pallid. His eyes were not red and suffused with blood, but had a peculiar lustre; and the pupils were much dilated. There was an unusual secretion of tears, and a discharge of mucus from his nostrils. His skin was cool, and covered with sweat. His tongue was white; and his bowels had been costive for several days. He complained of giddiness, headache, and lassitude. He was able to give some account of the two men he had left behind, but his recollection often failed him. Though he could stand erect and walk, his eyes appeared to deceive him, as to the relative situation of objects, and he stretched out his hands to direct himself. He had been unwell eight days; and his complaints began with languor, lassitude, giddiness, and headache, which had always continued, but had not before prevented him from attending to his usual duty. His general appearance approached nearly to that of a person affected with slight concussion of the brain, or in a bad state of typhus. By this, unfortunately, I was deceived; and, allowing myself to be guided by the state of the pulse, I did not bleed him. I ordered a large blister to be ap-

plied to the nape of his neck; he took eight grains of calomel, and a purging clyster was immediately given. Next morning (the 16th) he could scarcely give any account of his feelings; his pulse had become somewhat fuller, but did not continue regularly so during the whole day. I now observed some slight twitchings of the muscles of the face. He had several evacuations by stool, during the night; and this morning he vomited much bilious matter. The temperature of his body was now greater than natural, and his skin dry. I ordered his body to be sponged over with cold water; his head to be shaved; and snow and iced water to be applied to it. Though he was in a state of coma, this application produced such uneasiness, that it could only be continued for a few minutes. Towards the afternoon he became much worse; and the pupils, which continued dilated from the beginning, were now quite immovable. His breathing became very laborious; his pulse slow and irregular; he foamed at the mouth; and was occasionally convulsed.

Though I was convinced the favourable opportunity was past, I resolved to take some blood from him; and I accordingly opened the temporal artery. The effect surprised me; for, while the blood was flowing, his breathing became perfectly easy, which encouraged me to take a pretty large quantity. In the evening, however, his breathing again became laborious, the convulsions very frequent, and early next morning he died.

On opening the skull, nothing uncommon was discovered on the external surface of the dura mater. Its sinuses, however, were gorged with blood, and, the small veins, leading to the longitudinal one, much distended. Between the dura and pia mater, there was a small quantity of serum effused; and the surface of both, in the neighbourhood of the longitudinal sinus, was covered with that sort of matter which is observed on the peritonæum and other membranous parts which have suffered from inflammation. Adhesions had also taken place; and in two or three places small abscesses were formed. In the ventricles, and within the substance of the brain, nothing uncommon was found.

The treatment of the other patient fell to Dr. Calvert, and

was nearly the same as in the above, with the exception that he was not bled. He was admitted into hospital on the night of the 15th, and died on the evening of the 16th, having been unwell about six days. His death was more sudden than in the other, and was not preceded by the same violent symptoms. The pupils always showed more contractility.

On dissection, fewer adhesions were found between the membranes of the brain; but the same sort of matter was poured out on their surface; and this also more particularly contiguous to the longitudinal sinus, which was gorged with blood. The substance of the brain appeared more vascular, and was much redder than usual. In one of the ventricles there appeared a few red globules, diffused through the serum; which, in neither, exceeded the usual quantity.

Though we had been unsuccessful in the treatment of these cases, it affords me much satisfaction to have been enabled, by the appearances on dissection, to form a more correct idea of the nature of the disease than that generally entertained on the subject. In these cases, I suppose, from the slow and insidious progress of the disease, that the inflammatory action, during the first days, had not been very violent, and might, perhaps, have been what Dr. Paterson calls a *phrenicula*, or what nosologists distinguish by the name of *cephalitis;* for, during a whole week, they continued at their usual duties, and never complained until the disease had assumed that fatal and deceitful form peculiar to the *second* stage.

I can easily conceive that these symptoms have led many into error; and that, whatever may have been the treatment at the beginning, from mistaking the symptoms of effusion for those of the debility of typhus, barks and wine were immediately had recourse to, and thus hastened the fatal termination.

On the 31st of July the 62d regiment disembarked at Melazzo; and in a few days *this fever* made its appearance among the men. The above cases may be sufficient to show the appearance of the disease throughout the whole of the *second* stage; and, as out of 240 patients admitted into hospital, affected with it in every degree of violence, not one case terminated fatally, I shall endeavour to point out the symptoms

peculiar to the *first* stage of the disease, as they were observed in that number.

It did not appear to attack the robust and plethoric more frequently than the weak and delicate; though, in the former, its symptoms were always more violent. In the majority of cases it was ushered in with a cold stage; while some complained of giddiness and languor for several days previously to the accession of any regular paroxysm, which, however, was always a certain occurrence. When thus suddenly attacked, they complained of violent headache and giddiness, with throbbing pain in the temples, attended also with languor and lassitude. The pulse, in the early part of the disease, was full and strong, and often exceeded 100 strokes in the minute. The countenance was most commonly florid; but when giddiness had for a long time preceded the violence of the attack, it was pale and relaxed. The eyes, which were but seldom suffused with red, had a remarkable lustre, and were extremely sensible to light, which, as well as the least noise, gave great uneasiness. The bowels were always costive. The tongue was generally moist, and invariably covered with a white or yellowish mucus, and red about the edges. Though they generally complained of thirst on the first days of their illness, it never formed an urgent symptom; and the sensation entirely abated as the disease advanced. The skin, at the beginning, was hot and dry, the temperature often 102°; and, when the disease was about to terminate favourably, profuse sweating often occurred. Though there was in every case great drowsiness, approaching to stupor, they never enjoyed sleep. I never observed, even in the most severe cases, phrenitic delirium. They, on the contrary, lay motionless in bed and appeared uneasy when disturbed, or roused from their apparent slumbers. Nausea was always present; and in three cases it was attended with obstinate bilious vomiting. During the course of the disease, I have sometimes met with pulmonic symptoms.

It appears to me, that the cases which occurred, at the first appearance of the disease, were, *in general*, more violent than they afterwards proved to be, towards its decline, or when it was becoming to be less frequent. This took place about the

first of September; the weather had became cooler, the thermometer having fallen to 78°. There were also some showers of rain, with thunder, about this time.

Among the vulgar, both in Sicily and Calabria, most of the diseases peculiar to the climate, particularly those of a malignant nature, are attributed to what they call *mal' aria*. This language has even been adopted by some professional men; and I dare to say, the investigation of the nature and origin of diseases has thereby been not a little retarded. If it is meant to imply nothing more than *marsh miasma*, there can be no necessity for changing the phrase, particularly, as that substituted is by no means more philosophical or scientific; and as such change can only throw obscurity on a term, the import of which is already well understood. To this cause, then, the fevers prevailing in Melazzo and its neighbourhood, during the hot months of summer, had for a long time, been attributed; and it was solely from the information gained by dissection, that we have been enabled to trace, with much certainty, those fevers to the powerful action of the solar heat, occasionally aided by intemperance, violent exercise, and the like.

While the excessive heat prevailed, the character of the disease continued invariably the same; but, as I have already observed, about the 1st September, it not only became less violent in its symptoms, but also occurred less frequently. From the disembarkation of the 62d regiment, on the 31st July, until the end of August, it was common to see sixteen or twenty soldiers, affected with it, brought to the regimental hospital, in the course of one day; but, between the 1st and 10th September, only fourteen cases appeared; and, as the weather became cooler, with frequent showers and strong winds, it entirely gave place to diseases of a different nature.

I have only met with three cases in which it was followed by intermittent fever; and I have since met with one case, in which during the state of convalescence, the patient was seized with remittent fever, obviously observing the tertian period, and accompanied with severe diarrhœa. But it must be observed, that these were the only cases of the kind which did

occur; that they appeared at the close of the season, and were sufficiently distinct in their nature.

Soon after the disease appeared among the men, the necessary means were taken to counteract and avoid its causes; and in order to afford more room in the barrack, one company was sent on board a vessel anchored in the harbour; and it is worthy of remark, that though it continued embarked nearly during the whole time the regiment was quartered in Melazzo, a period of eleven weeks, it afforded only one solitary case. Even among the companies ashore, there was a striking difference with respect to the proportion of men attacked. In one of the quarters, the men slept in the walk surrounding the area of a convent, exposed to the open air, and during the day, they always availed themselves of the protection it afforded from the excessive heat of the sun. In this company only ten men were attacked, while in some other companies, which did not enjoy these advantages, the number exceeded thirty. These circumstances, which were so useful in preventing this company from being seized with the prevailing disease, exposed it, however, to those of a different nature. Diarrhœa and rheumatism were common in it; and it furnished more cases of ophthalmia than all the other companies put together.

Among the diseases affecting the brain, or its membranes, we must expect to find a strong analogy, and many symptoms in common. Those which the disease in question most resembles are cephalitis, phrenitis, and hydrocephalus internus.

I have already said, that (in upwards of 240 patients all soldiers,) I did not observe a single instance of wild and furious delirium. Even when the pulse was full and quick, giddiness and a tendency to deep sleep, were the symptoms which first caught the attention; they were the first complained of by the patient, and were always the most lasting. So far the disease differed from phrenitis, strictly so called. It must also be observed that, in the only two cases in which I had an opportunity of examining the appearances after death, the inflammation had not only affected the dura and pia mater, but had greatly injured the organization of the brain itself. There was congestion in the sinuses, and effusion of serum and coa-

gulable lymph. How far this complication of morbid appearances may associate the disease with cephalitis*, or account for the peculiar modification of its symptoms, I shall not take upon me to determine; neither am I very anxious to ascertain in what part of the organ the inflammation commenced, as such discovery could, in no degree, influence the mode of treatment.

It would, indeed be an endless, and not less an unprofitable task, to attempt to mark every accidental modification of disease by an appropriate name, or to assign for it a place in nosological nomenclature. "Imo etiam ubi distributio in species reperitur, id fit plerumque, ut hypothesi alicui, quæ veris phænomenis substruitur, suus reservatur honos; ac proinde ejusmodi discriminatio non tam ad morbi, quàm ad authoris ingenium, philosophandique theoriam accommodata est. Quantum medicinæ obfuerit ἀκριβείας hâc in parte defectus, ostendunt multorum morborum exempla; quorum nempe curationes hodie non desideraremus, si scriptores, dum experimenta et observationes medicas benevolo saltem animo communicarint, immo scilicet speciei morbus pro alio speciei diversæ substitutus non fefellisset, atque hinc etiam arbitror profectum esse, quod materia medica in sylvam tam immensam sed fructu perexiguo excreverit†."

With respect to hydrocephalus internus the analogy is not so great. It is produced by different causes, and is much slower in its progress. It is peculiarly the disease of early years; often appears hereditary; and is more certainly fatal. Like it, however, it is also marked by two distinct stages, as more particularly noticed in the cases which came from Archi. In hydrocephalus internus, the brain may suffer much from the pressure of accumulated serum before it produce death; in this disease, the quantity of effused serum was inconsiderable, while the destruction of parts by inflammation was very extensive.

Indeed, when an organ so important as the brain, on the healthy state of which, not only sensation, but the due perform-

* Though this term may be somewhat exceptionable, I have adopted it as it was necessary to make some distinction between *simple fever* and this disease; and as the former term conveyed very vague and erroneous ideas of its nature.

† Sydenhami Opera.

ance of every function depends, becomes the seat of disease, we must expect to find great derangement of the whole animal economy, the symptoms of the local affection not appearing merely as such, but as indicating general disorder of the whole system.

On this account it happens, that between the simple synocha and phrenitis, we can, with no degree of precision, draw the line of distinction*. To the same cause also it is, perhaps, owing, that in so many diseases described as simple fever, we find the affection of the head forming so conspicuous a feature; in many of which, I am convinced, this local affection is primary, and essential†.

Of this kind, I suspect, are many of the fevers peculiar to warm climates; in the production of which, the intense heat of the sun is allowed to have so considerable a share. Though such fevers belong almost exclusively to the warmer regions, they are also occasionally met with during the summer in more temperate climates. Their progress is, for the most part, quick and rapid, and they cease entirely, or at least are greatly modified, on the approach of autumn.

As I, therefore, have no doubt that the chief cause of the disease which prevailed in Melazzo last summer, and which has hitherto proved the most fatal among the troops in Sicily, was the intense heat of the sun, I have always considered the fever attending it as merely symptomatic of the inflammation within the skull. The manner in which the general febrile symptoms follow, with proportional severity, the affection denoting topical lesion of the organ concerned, is distinct and obvious; and the means had recourse to, and which prove most useful when directed to the relief of the local affection, procure also the most permanent and sudden cessation of the general febrile excitement. I have, therefore no hesitation, guided by the proofs afforded by dissection, in classing this disease with the phlegmasiæ.

* Wilson, Vol. III.

† May not the fever which lately prevailed at Gibraltar have been of this nature?

Other fevers, however, peculiar to these climates, and in which an affection of the head also forms a conspicuous symptom, undoubtedly belong to the tribe of intermittents; and widely differ from the disease in question. In the writings of Torti, they are particularly described under the title of *Febres perniciosæ comitatæ;* and in book iv. chapter 3. of his *Therapeutice specialis*, we find a collection of cases in which it took place. The fever, which this symptom attended, belonged to the type of tertian remittent, and it was never observed to occur until the *fifth or seventh day, and often at a more advanced period.* It therefore obviously formed a complicated disease, produced by a pure phlegmasia affecting that particular organ, supervening at some uncertain period of the original fever. Bloodletting was often had recourse to in order to relieve the immediate urgency of this symptom; but it was soon followed by doses of bark, by which the cure was finally effected.

In the disease, however, which prevailed in Melazzo, the cause and effect were simultaneous; for, whether the patient suddenly fell motionless on the ground, or complained only of headache and giddiness, these symptoms, whatever interval might take place, I may say invariably preceded the formation of the febrile paroxysm.

In as much as relates to the *period*, at which the affection of the head appeared, it approached more closely to the *causus*, or *febris ardens*. "Erat enim constitutio febrium ardentium in hoc modo. *Ab initio* soporosi, anxii, horridi; febris acuta, non valde siticulosi, non deliri. De naribus parum stillavit. Exacerbationes plerisque diebus paribus," &c.* These also were most commonly met with during the summer season; they affected chiefly young robust people who used much exercise, or happened to be travelling, and did not sufficiently guard against the influence of the sun; or such as exceeded in the use of hot stimulating diet; and, in certain habits of body, they were often complicated with cephalitis and phrenitis.†

The cases here quoted are, however, referred to the "*con-*

* Hippocrat. 1 sec. 3 lib. 3 epid.

† Burserii Opera, vol. ii.

tinua remittens." In the summer season they showed a stronger disposition to preserve the nature of ardent fever than towards autumn, particularly if intermittents prevailed, or after the inflammatory diathesis, with which they were at first complicated, had been subdued.

It would almost be endless to describe every variety of fever of this kind mentioned by authors. I shall, therefore, advert only to another; which, I am inclined to think, was a disease of the same nature as that observed in Melazzo during this and former years. It is to Lucian we are indebted for this singular record. The disease appeared at Abdera, during the middle of summer; it was evidently produced by the intense heat of the sun; its attack was sudden, and not one in the whole city escaped it. The delirium which attended this disease was not of the furious and ungovernable kind; and it terminated about the seventh day, with profuse sweating, or hemorrhagy from the nose. The circumstance which, in the relation, most strikes our notice, is the pertinacious manner in which the mind pursued the same train of ideas which had grown familiar by habit, or had been made powerful by strong momentary impressions. It is said to have first broken out among the citizens at the theatre, where they had assembled at noon, while they attended, with admiration, to the performance of one of the plays of Euripides.

The story, however, is perhaps too well known to require to be related here.

Taught, as I was, by the fatal issue of the cases which came from the telegraph, I could not long hesitate as to the method to be pursued in the treatment of this disease. Some slight cases, as at first, were cured by cold affusion and purging; but to these remedies alone I very seldom trusted. Copious bloodletting was what I chiefly depended on. In the use of this remedy, I was never entirely directed by the state of the pulse, particularly after some days had elapsed, the headache and giddiness appearing more certain guides; and I often found, that, in such cases, these symptoms were relieved even while the blood flowed, the pulse gradually becoming fuller, and more regular.

It is perhaps difficult to determine, whether bleeding from the temporal artery ought *always* to be preferred to venesection at the arm. Both, indeed, will afford immediate relief for the headache and giddiness; but I think that obtained by bleeding from the temporal artery is, *in general*, *cæteris paribus*, more permanent.* Small bleedings, however, from the temporal artery, are peculiarly applicable to protracted cases, where slight headache and giddiness continue after the removal of the general febrile symptoms: and in which such bleedings at the arm, as could produce any relief, would be inadmissible.

As to the quantity to be taken, I have always regulated it by the relief obtained. In a young man, whose pulse was by no means full or hard, but where there was much giddiness and tendency to coma, I took away nearly forty ounces of blood from the temporal artery at one bleeding. In two days he was able to walk about, and quite free from giddiness. Indeed, it appears to me, that profuse bleeding, in this disease, does not leave such permanent debility as in the other phlegmasiæ.

Blistering the head I find another powerful remedy, which ought never to be neglected; and in the circumstance of long continued giddiness just mentioned, it is of the utmost service. It ought to be frequently repeated.

There is always a great tendency to costiveness in this disease, which ought to be removed. I have therefore made it a rule, observing the relief it produces, to keep up a gentle diarrhœa as long as the symptoms continue at all violent.

Under the head of cold affusion, may also be comprehended cold drink, and cold applications to the shaved head. They are

* I have met with some Sicilian physicians, well acquainted with the nature of this disease. Their method of cure consists chiefly in the application of leeches to the temples, and the use of cold acid drinks. Opening the temporal artery is preferable, as it combines the effects of general and local bloodletting. In the army, bloodletting was first practised in the summer of 1807, in one of the regiments of footguards, with much success; but it required the proofs afforded by dissection, to render that practice general; which did not happen until the year 1809. " With respect to opening the temporal artery, it may be here added, that this mode of bloodletting was practised by Dr. Irvine, in the summer of 1808, with much success.

powerful auxiliaries in lessening the violence of excitement, and never fail to be extremely grateful to the feelings of the patient. I have applied pounded ice to the head; but it produces too much pain to be long endured, and, on that account, cold water may be preferable.

I have not been in the habit of using these remedies in daily succession; but, as it were, all at the same time; as I am convinced that a speedy recovery depends entirely on attacking the disease vigorously in the beginning. Indeed, when this circumstance was duly attended to, it never proved tedious; and the patient was almost always able to get out of bed on the fourth or fifth day.

As the violence of the disease abates, the pulse becomes slower, full, soft, and equal; and I have seen a great number of cases, in which its slowness was very remarkable during the state of convalescence, not exceeding forty strokes in a minute. The tongue also becomes clean, and this is a sure criterion of recovery. Giddiness is always the most lasting symptom; and is pretty steadily accompanied with dilated pupils, and both symptoms generally disappear together.

I have only met with four cases of relapse. In one of them, which was very tedious from the beginning, and in which dull pain in the occiput and severe giddiness were constant symptoms, I gave digitalis and calomel.* In one patient, who had been much addicted to the use of ardent spirits, and had suffered from the repeated use of mercury, after the general febrile symptoms had been removed, there appeared, for some time, a tendency to chronic mania, which, however, disappeared under the use of nauseating doses of antimonium tartarisatum. In the three cases which, at the close of the season, terminated in intermittent fever, I gave bark; and to that remedy the fever readily yielded.

* This patient got well soon afterwards, but I do not attribute his recovering to this mode of treatment.

On the Connexion that subsists in certain cases between Amenorrhœa and Phthisis Pulmonalis, as cause and effect.

By WILLIAM SHEARMAN, M. D.

From the Edinburgh Medical and Surgical Journal, for January 1810.

Dr. Cullen has asserted, in his description of phthisis pulmonalis,* that "in the female sex, as the disease advances, and sometimes *early in its progress*, the menses cease to flow; and this circumstance is to be considered as commonly the *effect*, although the sex themselves are ready to believe it the sole *cause* of the disease." There is some ambiguity in this expression; for if the suppression occur as the *disease advances*, how can it possibly be mistaken for the *cause* of the symptoms that *previously* appeared, even by the sex themselves? I apprehend it could not be unknown to Dr. Cullen, that in many instances of pulmonary consumption, a suppression of the menses does certainly *precede* every appearance whatever of pulmonary affection; and it is an interesting subject of inquiry, whether the pulmonary disease is not occasioned by the suppression? and it may be useful to ascertain whether, in certain cases, amenorrhœa does not prove a cause of phthisis in the predisposed? or rather, whether, in some cases of amenorrhœa, an affection of the lungs, resembling phthisis, and sometimes terminating in it, is not one, among several other effects produced by, and dependent upon, the suppression of the menses; to which suppression, therefore, we are to direct our attention, as forming the chief disease, upon the removal of which all the other symptoms will vanish, provided that can be effected before the lungs shall have sustained such organic lesion as to render them incapable of continuing duly to perform their proper functions, so essential to life and health? The progress and connexion of the various symptoms will, perhaps, be best illustrated by a case which occurred to me not long since. A young woman, aged 20 years, of a very delicate habit, but in pretty good health, had experienced about five months ago, an ob-

* See First Lines of the Practice of Physic.

struction of the menses in consequence of cold; from this time her health gradually declined, she became languid and weak, had frequent pains in her side, and occasionly a short dry cough. Her appetite was impaired, and she became rather emaciated. In the month of July I first saw her; at this time she complained of pain in the left side, occasional fits of coughing, which recurred at uncertain intervals, but generally with considerable violence night and morning. She had scarcely any expectoration, little thirst, pulse 120, appetite impaired, bowels regular, countenance and lips very pale; there had been no return of the menses since their first suppression. A blister was immediately applied to the pained part, and directed to be kept open; five grains of rubigo ferri, combined with myrrh and castor, to be given three times a-day, with infusion of chamomile flowers, and the feet to be immersed in warm water every night. The pain was removed by the blister, and it was healed up. The medicines were continued about six weeks; during this time the pain occasionally recurred, which was always soon removed by a blister. The cough was sometimes considerably better, sometimes aggravated, with trifling expectoration; her pulse was diminished to 90; her appetite improved, and her countenance looked better. I now discontinued my attendance, and have since learned, that the only remedies she afterwards used were expectorants and blisters, occasionally, when there was cough or pain, and she *entirely omitted* all other medicines. She continued without much alteration, excepting that she gradually grew weaker, and within the last fortnight was almost confined to bed, without, however, the cough or expectoration being greater than before. On the Saturday morning previous to her death, she was seized with violent excruciating pain in the abdomen, particularly about the umbilicus, which continued, with more or less violence, for several hours; it then left her, and she seemed much in the same state as before, until the Thursday morning, October 13th, when, soon after having got out of and into bed, without assistance, she suddenly expired.

On opening the thorax, there was found a very general adhesion of the pleura pulmonum to the pleura costalis, yet not

so firmly but they might be easily separated by the fingers; there was a small quantity of fluid in the cavity, not exceeding three or four ounces; the lungs were very dense, and of a dark color, resembling the liver in appearance; portions of them, when put into water, fell to the bottom. Being cut into, some of the bronchial cells contained a purulent matter, but in small quantity, and here and there was a small excavation the size of a pea; a few small tubercles also were found, not inflamed. The external surface of all the small intestines was very red, and seemed to be studded with small red points, resembling the villous coat of the same intestines when injected. There was no appearance of gangrene, neither was there any adhesion between the different convolutions of them, nor between any of them and the peritonæum; the omentum also was extremely vascular; the mesenteric glands were enlarged and hardened in several places; the pancreas, spleen, and other viscera, were of their natural appearance.

That the lungs were injured in a great degree, was evident from the tubercles found in them, and from the pus contained in the bronchial cells, but still the ulceration had not arisen to such a height as to be the immediate cause of death. More extensive affections than the present one are perhaps frequently recovered from, and tubercles may remain for many years with impunity, if no exciting cause of inflammation arise. Although, likewise, there were marks of inflammation upon the intestines, yet this inflammation did not immediately destroy the patient; for the pain had left her some days previous to her death, indicating a cessation of the inflammation; which, as it was not followed by gangrene, cannot be considered as the cause of death. The enlarged state of the mesenteric glands affords a presumption, that the flow of chyle into the bloodvessels might have been interrupted, but certainly there was not sufficient appearance of emaciation in the dead body, to enable us to attribute her death to this circumstance. We must look for the cause of death, not in the affection or destruction of any one particular organ, but in that state of general debility, consequent upon the interruption of a very important function in the female economy; during which state of debility, parts of

the body peculiarly predisposed are very apt to take on disease. In the present patient, there appeared to exist a scrofulous diathesis, wherein the lungs and mesenteric glands are always readily susceptible of disease, from the slightest causes, and always the more readily the greater the state of weakness in the system, however induced. Every kind of weakness is accompanied with irregularity in the action of the living power; hence spasmodic contractions of various parts. This disposition to preternatural contraction is particularly observable to take place in the bloodvessels, in all cases of amenorrhœa, forming the principal symptom in what has been called chlorosis, differing from hectic fever, chiefly, in depending upon a morbid action of the vessels themselves, independent of the absorption of extraneous acrimony. This state of the vessels is perhaps at first produced by plethora, arising from a retention of a quantity of blood, which excites the larger arteries to stronger action, while yet the resistance in the extreme vessels in the uterus is not overcome by its increased action, long continued, exhausts the living power, and produces weakness. During this state of general weakness, and morbid action of the bloodvessels, partial inflammations are very apt to arise. No wonder, then, that the lungs, when, from constitutional predisposition, they are so susceptible of disease, should be one of the first parts to take on inflammation. If by suitable remedies the lungs are relieved, (the disposition to general inflammation still continuing from its orginal cause, the suppression not being removed) some other part is very readily affected; hence in the present case arose the inflamed state of the intestines, during which all the thoracic symptoms disappeared. That this irregularity of action in the vessels, sometimes exciting symptoms of inflammation in one part, sometimes in another, entirely depends upon the suppression of the menses is evident, from its generally ceasing upon the return of that evacuation, and from the perfect restoration of health immediately following it. No occurrence is more common than the attack of cough, pain in the side, and difficulty of breathing in females, soon *after* the obstruction of the menses, and upon their recurrence all these symptoms going off. If this view of the case is founded in fact,

does it not follow, that in pulmonary complaints, arising from suppression of the menses, much mischief may be done, by employing, to any extent, the strict antiphlogistic treatment, to remove these secondary symptoms, since it evidently tends to increase that debility which originally produced them. There can be no doubt, but that when the affection of the lungs has proceeded to a certain extent, it will go on independently of its cause, and is no longer to be removed by the restoration of the suppressed evacuation, but requires the same treatment as if it arose from any other cause whatever. It is in the earlier stages of the complaint, where it is consequent upon the suppressed discharge, that I would direct my attention to the suppression, as the chief disease, and merely palliate the troublesome pulmonary symptoms; and experience has convinced me how easily this may be effected by the application of blisters, repeated according to circumstances, while we are endeavouring to reproduce the suppressed evacuation by appropriate remedies, and to restore strength to the system by the use of tonic medicines and nourishing diet. There is a natural tendency in this *chronic general inflammation* to wear out itself, but as this requires many months, and as, during that time, much mischief may arise in various parts, it is always proper to endeavour to remove it, which is most effectually done by restoring the natural discharge from the uterus. The almost constant failure of success we meet with in our treatment of incipient phthisis by evacuations and debilitating medicines, will justify us in recurring to an opposite mode, founded upon the opinion of its being in many instances a symptomatic affection only. Dr. Beddoes observes, " In some instances, when the fox-glove has removed the hectic fever, and greatly reduced the *expectoration and cough*, the decline shall become almost imperceptible, the patient *frequently appearing chlorotic*, but being really phthisical, as the *event* most commonly, and *sometimes* dissection, have evinced." This serves to confirm the opinion, that the pulmonary symptoms are not in these cases the original idiopathic disease. If it be objected, that this doctrine can only be applicable to incipient phthisis in *females*, I would appeal to the experience of practitioners to determine, whether,

in the female sex generally, the approach of phthisis is not more insidious, and its progress more slow than in men, where we usually find the attack more marked, and immediately consequent upon some obvious occasional cause, the symptoms sooner becoming more violent, and its whole progress more rapid. That this difference in the appearance of the disease arises chiefly from its frequently being rather symptomatic than idiopathic in females, will be apparent, if we find, that when phthisis arises in them from some obvious occasional cause, and where there has *never occurred suppression of the menses*, its attack and progress are more marked and rapid, as in the male. That it is so, I think experience will justify me in asserting; and impressed with this opinion, I cannot but recommend that we should keep in view the probable dependence of the pulmonary symptoms upon the interrupted functions of the uterus, and direct our treatment accordingly,* at least, until we are convinced upon trial, that the opinion itself is fallacious, and the proposed treatment inefficacious or prejudicial.

London, 21st July, 1809.

* The frequent success of Griffith's myrrh mixture, so much used formerly, may perhaps depend upon its emmenagogue powers; it certainly was serviceable in a greater proportion of females than males labouring under pulmonary complaints.

SELECTED REVIEWS.

The MERCURIAL DISEASE. *An Inquiry into the history and nature of the disease produced in the human constitution by the Use of Mercury, with observations on its connexions with the Lues Venerea.*

By ANDREW MATHIAS, Surgeon Extraordinary to the Queen, &c. &c.

From the London Monthly Review, for February, 1811.

THE object of this inquiry is highly interesting, as the development of a series of curious physiological facts, and still more as likely to have a material influence on medical practice. Mr. Mathias attempts to prove that, when mercury has been received into the constitution in too large a quantity, or under unfavourable circumstances, it has a tendency to create a specific and formidable disease; and that, while this process is going forwards, all the salutary effects of the mercury are suspended, or counteracted. It would appear also that this is by no means a rare occurrence, but one which is every day taking place under our inspection, although we have unfortunately ascribed the effects which are produced to a different and frequently to an opposite cause. In the introduction to his discussion the author gives a brief analysis of the plan which he proposes to pursue, and of the positions which it is his object to establish. After having shown that the disease exists, and that it is caused by mercury alone, he proceeds to state that it possesses a uniform and specific character, and that it is distinct from other diseases; concluding from this circumstance that it must have an important effect on our method of treating syphilis.

According to this plan, the first section is intitled a 'general history of the mercurial disease,' and consists of an ample and perspicuous account of its symptoms, both local and general, of the manner in which it makes its first attack, and of the course which it usually maintains. It might naturally be expected that a disease, so formidable as that which is produced by mercury appears to be, could not altogether have escaped

the notice of former practitioners, though they might have been mistaken as to its cause, and Mr. Mathias shows this to have been the case. Mr. Hunter's accuracy of observation could not overlook such remarkable effects as those which are detailed in this work; and he accordingly stated many instances in which they took place to a great extent, and in which he was fully aware of a deviation from the usual course of things, with respect to the operation of mercury on the system. He even went so far as to suppose that the mercury itself was concerned in the production of the symptoms: but it seems that he went no farther, and did not arrive at the result which the present author attempts to substantiate, that the mercury alone, without any united operation of the syphilitic poison, generates a separate and uniform disease. Observations similar to those of Mr. Hunter were made by Dr. Swediaur, and by Mr. Howard: but they in like manner stopped short, without reaching the important conclusion which is so explicitly brought into view by Mr. Mathias.

The great point which it behoved the author to establish, and which must serve as the basis of all his theoretical and of a considerable part of his practical reasoning, is the fact that the specific disease, which is described in this work, can be produced by mercury independently of the conjunction or interposition of any other morbid affection. It happens, however, that cases proper for the establishment of this position do not frequently occur, because it is seldom that mercury is taken in a sufficient quantity to excite its appropriate symptoms, without some other disease being present, which might be supposed to have a share in their production. Yet instances of the simple effects of mercury are occasionally found, and two are here detailed which appear to be fair examples of this kind; in which a train of symptoms ensued, both local and general, corresponding with the affection specifically styled by the author, *the mercurial disease*. At the same time that we give this opinion, we think that it would be extremely desirable to place so very essential a point beyond the reach of the slightest uncertainty; and we hope, now that the subject is explicitly brought into discussion, that more occurrences of

this kind will be noticed. It is a singularly embarrassing circumstance, and one which has principally tended to keep the matter so long in obscurity, that the mercurial disease, in all its leading symptoms, very strongly resembles syphilis; and since it is in a great majority of cases produced by the mercurial course which is adopted to remove syphilis, those appearances which should have been attributed to an excessive quantity of this medicine, have been ascribed to a deficiency of it. It is farther remarkable, and in a practical point of view extremely material, that, as soon as mercury begins to excite in the constitution its particular morbid effects, its antivenereal powers cease, and it has no influence over this complaint, until the constitution becomes entirely relieved from its *mercurial* effects. Proceeding on this hypothesis, every practitioner must immediately come to the conclusion, that a very large proportion of all those inveterate and deplorable cases, which were supposed to be syphilitic, but which mercury was found insufficient to remove, were really cases of 'the mercurial disease,' succeeding to an excessive or improper use of mercury; in which the venereal virus was either extinguished, or at least in a dormant state; and which were to be relieved, not by continuing the use of mercury, and even pushing it to a greater extent, but by entirely laying it aside. When we reflect on these facts, for facts they certainly appear to us to be, we shudder at the imminent danger in which the lives of so large a proportion of our fellow creatures have been placed; and we can scarcely feel sufficiently grateful to the sagacity and assiduity which have developed and established such important truths.

After having taken a general view of the disease, Mr. Mathias describes more minutely the local symptoms which it exhibits, the bubo, the chancre, the affection of the throat, and of the bones. We shall in general observe, on this part of the work, that the author expresses himself with clearness and precision; and so far as internal evidence can enable us to give an opinion, we should regard his account of the different forms which the disease assumes, as a faithful representation of what has actually passed under his observation. We before

remarked that the mercurial disease attacks all the parts which are the seat of syphilis; and it attacks them nearly in the same order, and assumes very nearly the same external appearances. The only mode, by which they can be distinguished, is by minutely attending to the previous history of the disease, to the effect of remedies and particularly of mercury on it, to the probable exciting causes, and to the progress which it is making. By the assistance of all these circumstances, a tolerable judgment of the case may in general be formed: but still many instances must occur in which the only deciding point will be to observe whether mercury retards or promotes the cure, and whether, when mercury has been given without effect, the symptoms are not alleviated by intermitting it.

As in the discrimination between the mercurial and the venereal affections, we are frequently obliged to assist our judgment by inquiring into the causes which operate in the production of the former, it is of great importance to make ourselves intimately acquainted with these causes. Mr. Mathias accordingly devotes a section to this subject, and enumerates at some length those which he conceives to act the most powerfully in this way. The causes which he states are partly constitutional, and partly local or accidental. A peculiarity of habit sometimes exists in which mercury acts as a poison, and almost immediately tends to the production of the mercurial disease, when given even in the smallest quantity. A plethoric state of the body, great nervous irritability, and the presence of scrofula or scurvy, seem also to favour this affection: but it is more frequently to the accidental causes that it owes its origin; and of these the most material are improper treatment during a mercurial course, the topical application of stimulating mercurial preparations to venereal sores, and the employment of the saline preparations of mercury instead of the simple pill or ointment.

The outline which we have given of the author's doctrine and opinions will render it sufficiently apparent, that a new light has here been thrown on the management of mercury and the treatment of syphilitic complaints; that in some instances, in which we were before involved in difficulties, we

have now a clear principle to direct us; and that a considerable change in our practice must be the result. A section is allotted by Mr. Mathias to this point; the great object of which is to show the importance of guarding against the exciting causes of the mercurial disease, and to intermit all mercurial applications as soon as it makes its appearance. We have afterwards some very judicious remarks on the operation of other supposed antivenereal medicines, mezereon, sarsaparilla, guaiacum, &c. The general conclusion is that they have no power over the syphilitic virus, but that they may be of use in removing the *mercurial disease;* and by an attentive examination of the cases in which they have been supposed to be serviceable, Mr. Mathias's opinion is rendered extremely plausible. It is highly probable that, in these instances, the syphilitic affection was subdued, and the mercurial disease alone remained. In this part of his work, the author has displayed a considerable share of acuteness and discrimination.

The last section treats on the cure of the mercurial disease; which consists fundamentally in refraining from the use of mercury as soon as the least indication of the existence of the disease takes place, and, which is one of the first circumstances to be noticed, as soon as the mercury ceases to produce any farther benefit with respect to the syphilitic symptoms. If the system be plethoric, the antiphlogistic regimen is to be pursued in all the different ways; if it be in an irritable or reduced state, we are to give a proper quantity of nutrition, and cautiously to administer tonics. Excepting occasionally, when topical applications are necessary, the above treatment appears to be all that is essential.

In concluding our remarks on this volume, we must express the great pleasure and improvement which we have derived from the perusal of it; and we have no hesitation in saying that it must produce an important change in practice, and must tend to facilitate the cure of some of the most unfortunate states of disease to which the human body is subject. As an addition to the other merits of the performance, we must not omit to mention the candor with which the author speaks of those who differ from him in opinion; a conduct which displays an amiable disposition and a cultivated understanding.

[From the Edinburgh Review, for November, 1810.]

A Letter on the Differences in the Structure of Calculi, which arise from their being formed in different Parts of the Urinary Passages; and on the Effects that are produced on them by the internal Use of Solvent Medicines, from Mr. William Brande to Everard Home esq. F. R. S. (From the Philosophical Transactions for 1808, Part II.)

Some Observations on Mr. Brande's Paper on Calculi. By Everard Home esq. F. R. S. (From the same volume.)

An Account of a Calculus from the Human Bladder, of uncommon Magnitude. By Sir James Earle, F. R. S. (From the Phil. Trans. for 1809, Part. II.)

Observations on the Effects of Magnesia in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr. William T. Brande, F. R. S. to the Society for the improvement of Animal Chemistry, and by them to the Royal Society. (From the Phil. Trans. for 1810, Part I.)

On Cystic Oxide, a new Species of Urinary Calculus. By William Hyde Wollaston, M. D. Sec. R. S. (From the same Work, Part II.)

THESE curious and valuable papers relate to one of the most excruciating torments with which the Divine Providence has, for wise purposes, decreed that the lot of man should be mingled, during his existence in this world. Indeed, were it not that custom teaches us to consider mental afflictions as, in the general, more severe than any bodily pains, (and, no doubt, to some natures, they are beyond all comparison, more unbearable,) we should be disposed to rank the dreadful complaint which we are about to treat of, as the worst of human maladies. To investigate the nature of the Stone, for the purpose of discovering solvents which might remove it, has accordingly been long considered as one of the noblest problems in practi-

cal chemistry, and among the best services which that science could render to the healing art. It may be remembered, that to this investigation we owe the brilliant discoveries of Dr. Black, which, by disclosing the nature of fixed air and of the alkaline earths, paved the way for all the modern improvements in chemical knowledge. Those great acquirements in speculative science were the compensations which he obtained for his disappointment in a search far more important and interesting to humanity: as the navigators of the fifteenth and sixteenth centuries discovered the New World, and explored its strange recesses, while occupied in the pursuit of an earthly paradise, or a fountain of health and youth. Even the legislature of this country took some part in those inquiries, and gave rewards to the discoverers of solvents. The most noted instance is that of Stephenson; and here the money was granted upon the faith of a cure *said* to be performed, the body not having been examined after death. We need scarcely add, that medicine thus patronized, and, as it were, privileged, has long since been completely exploded; and though, in its failure, the parliament resembled the celebrated philosopher whose name we have mentioned, we do not remember that its investigations were like his, rewarded by any other discoveries.

The hopelessness of the search for *solvents*, has lately turned the attention of the chemists and physiologists to another problem; and they have been occupied in ascertaining the process of calculous formations, with the view of administering such *preventive* medicines as may indispose the system to produce those concretions, or check their growth, without altering the tone of the constitution. The papers now before us throw considerable light on this important subject; and contain also a good deal of information, tending to elucidate the fallacies of the older doctrines respecting solvents. Indeed, did we not know the danger of trusting to medical reports of cures, and did we not recollect how regularly a series of cases, all quite apt and most nicely fitting the doctrines, forms a part of every modern treatise on medical subjects, we should be tempted to consider a great part of the question solved with regard to the stone, and should confidently look to the extirpation of that

worst of maladies. Even moderated by such unpleasant recollections, as we own our hopes to be, we cannot look to the eminent names prefixed to these tracts without great respect; and we feel at least so much inclined to listen, that we shall make no farther apology for laying before our readers an account of what the papers have added to the stock of our knowledge on this subject.

In order to convey some idea of the greatest degree in which this disease has ever yet, we believe, been known to afflict its wretched victims, we shall begin with noticing the case narrated in sir J. Earle's paper.

Sir Walter Ogilvie, a Scotch baronet, received, at the age of twenty-three, a severe blow on the back, from the boom of a vessel; and the pelvis and lower extremities were in consequence paralyzed. He remained in bed for fourteen months, in an horizontal position; and, during the first two months, his urine was of necessity drawn off by a catheter. After this, he had somewhat recovered the use of his limbs, and could walk or ride, though with difficulty: his general state of health, too, was weak and precarious. About twenty years after the accident, he was afflicted with symptoms of stone; and upon examination, a calculus of considerable size was found to have been formed in the bladder. An operation was now recommended, but, unfortunately, delayed from time to time, although the patient's health constantly declined, and the irritations and pains in the bladder greatly increased. In this melancholy state he continued for eight years longer, when he became unable to make water in an upright position: he was obliged to place himself in an inverted posture, that the upper part of the bladder might become the lower; and as, even by this means, very small quantities of urine only could be voided, the irritation of endeavouring to make it became almost perpetual, and he was obliged to perform the above mentioned operation every ten minutes, with scarcely any relief. The principal remedies which he tried were *aqua calcis* and *uva ursi*. He used opium also to alleviate the pain; and this, adding to a naturally costive habit, rendered aperient medicines frequently necessary, which, again, must have increased

the irritations of the disease. At the age of fifty-three, thirty years after the accident, the spasms became so constant and so violent, that he resolved to have the operation performed; and the paralysis of the lower parts being deemed no obstacle to it, he was conveyed by sea to London, where sir James Earle and Mr. Cline consulted upon his truly lamentable case, about the end of July 1808.

Upon examining the lower part of the belly, a tumor was perceived, so large, that sir James Earle doubted its being altogether caused by the stone; but, on sounding, the instrument would not enter the bladder; and further examination showed, that the whole cavity was nearly filled with calculus. It was clear, therefore, that this could only be extracted, if it should prove soft enough to admit of being broken, and thus brought away piecemeal; and it was deemed impossible to attempt the operation above the *os pubis*. The ordinary method was therefore resorted to; and after some days' consideration, sir Walter Ogilvie, well aware of the difficulties of the case, and of its great uncertainty, yet justly weary of the long life of torment which he had led, was resolved to have the operation performed, for the chance which it afforded of bringing a relief, only otherwise to be attained by death. It was accordingly performed by Mr. Cline, on the 11th of August.

He was placed in the usual position, and the ordinary ligatures were applied; but the paralysis of the lower limbs rendered it unnecessary to confine them, and they were left at liberty. It was found impossible to pass the staff, or any kind of forceps, into the bladder; but on pressing hard with the finger, part of the stone felt soft, and gave way. This making room for the forceps and scoop, some parts of the concretion were brought away; and, in this manner, about a large teacupful was extracted. The great mass, however, continued hard and immovable: it could neither be broken nor shaken from its position; and after trying every instrument, of all powers, the operator was compelled to desist, leaving it (with the exception of the soft matter brought out) in its former state. The patient bore this long process with great fortitude: happily, the paralysis of his lower regions may be supposed to have some-

what diminished the sensibility of the parts. He was, however, completely exhausted, from weakness and fatigue.

Although no hemorrhage, nor any fatal symptom, appeared to result from the operation, it had afforded no sort of relief; and the spasms continued as before. They soon increased in frequency and effect, as he became exhausted, and unable to stand them. He seems to have had a gleam of hope on the morrow of the operation; for he frequently inquired when he might be well enough to have the remainder extracted. But he gradually sunk under the repeated and violent irritations of the spasms On the eighth day, his pulse became smaller and quicker, and he could scarcely be prevailed on to take any sustenance: on the ninth, he was more impatient, feverish and restless; and on the tenth day, "he desired not to be teased to take any thing more; when covering himself completely with the bed-clothes, he quietly resigned a most singularly miserable existence."

On dissection, the bladder was found to be *quite filled* with a huge calculus, composed of the triple phosphate, which we shall afterwards have occasion to speak of; and of such an unusual proportion of animal matter, that it speedily showed a strong tendency to putridity, and even engendered maggots. It weighed forty-four ounces, or three pounds four ounces, apothecary's weight. Its form was that of an irregular ellipsoid; the larger circumference being sixteen inches, and the smaller fourteen. It had filled the bladder, and taken its shape, but apparently without distending it. The ureters had become enlarged, so as to contain the urine, and act as small bladders; while the large bladder only permitted that fluid to pass off by means of small furrows which it made in trickling along the surface of the stone, between the stone and the badder. There appears no reason to doubt, that, in a short time, even this painful and insufficient operation would have become impracticable, by the further enlargement of the stone stopping up the mouths of the ureters; in which event, death must have ensued from suppression.

Such, in its very worst state no doubt, is the dreadful malady which forms the subject of the chemical papers now be-

fore us. To these we must now direct our attention; and we can promise the reader sufficient reward for his trouble, if he shall be pleased to follow us through the analysis of their contents.

Mr. Brande's valuable paper, which stands first in the prefixed list, is founded on an examination of the collection of calculi contained in the Hunterian museum. Beside the known richness of that collection, most of the specimens have histories of the case annexed to them, which are obviously of the greatest use in the investigation of the subject. Mr. Brande arranges calculi into four classes: first, Those which are formed in the kidneys, and voided without undergoing any change in the passage; second, Those which are retained in the kidneys; third, Those which are found in the bladder; and, fourth, Those which are found in the urethra. On each of these species, our author has made a number of experiments; and the results exhibit a sufficiently distinct account of their chemical analysis. The first class, or calculi of the kidneys, consist almost always of uric acid (the Lithic acid of Scheele) and animal matter; the animal matter is found in various proportions, from one-eighteenth to one-third of the whole compound. Sometimes the acid and the matter are not formed into a solid calculus, but pass off mixed with phosphates. The author, however, is disposed to think that the phosphates are not elaborated in the kidney, but mingle with the calculous matter in its passage. In some few instances, the calculus contains oxalate of lime; but this is very rare. The size of these calculi varies, and one has been described by Dr. Heberden, so large as to weigh twenty-eight grains; but it may have been detained on its passage, and so augmented in bulk. The largest which Mr. Brande examined only weighed seventeen grains. All the calculi of this class are certainly soluble in pure potash; but it is needless to add, that the caustic qualities of this substance render its application in the living subject impossible.

The second class is formed by the detention of a calculus in the kidney, while either the secretion of uric acid enlarges its bulk, sometimes so as to form a complete cast of the pelvis of the kidney; or the stream of urine to which it is exposed deposits the phosphates upon the original nucleus of uric acid and

animal matter. In the one case, we have an enlarged calculus of the first class: in the other, a calculus of the third class, only detained in the kidney.

When the uric calculus passes into the bladder, and is there detained, or when a calculus of oxalate of lime comes down from the kidney; or when portions of sand or animal mucus are deposited in the bladder; or when any extraneous substance has found its way thither and is detained—in all these cases, that is, on all these substances as nuclei, there are formed calculi (commonly termed stones in the bladder) with greater or less rapidity, according to the tendencies of the constitution. They may be ranged into three subdivisions: those composed chiefly of the uric acid; those chiefly composed of phosphorates (the ammoniaco-magnesian phosphate, or phosphate of lime, or a mixture of the two;) and those containing oxalate of lime, which are commonly termed mulberry calculi. Messieurs Fourcroy and Vauquelin, and Dr. Pearson, having found the greater number of calculi to be composed of uric acid (that is to say, belonging to the first of these subdivisions), our author was surprised to find that, in the Hunterian collection, this proportion did not hold; for, of a hundred and fifty which he examined, only 16 were composed of uric acid wholly (beside the animal matter:) 45 of that acid with a small portion of phosphates: and 5 of the acid with phosphates and nuclei of oxalate of lime; while 66 contained phosphates with a small admixture of uric acid; 12 the phosphates entirely; and 6 chiefly oxalate of lime. Observing that a greater loss was sustained in obtaining uric acid from calculi in the bladder, than from calculi in the kidneys, Mr. Brande was led to conclude that the former contained urea. With a view to ascertain this point, he made some experiments, and found that an uric calculus, weighing 60 grains, contained 5.2 of urea and muriate of ammonia, 6 of ammoniaco-magnesian phosphate, and 48.8 of uric acid: but no urate of ammonia could be detected: from whence, and from other experiments, our author infers, that this substance, when supposed to exist in calculi, owes its origin to the development of ammonia, and its recomposition with uric acid in the course of the processes of decomposition. The mulberry

calculi (which are rare) contain, in general, a considerable admixture of phosphates and uric acid with the oxalate of lime. The very large calculi often contain an agglomeration of different species of calculi. Mr. Brande examined one, of twenty-three ounces and twenty-six grains, which had a nucleus of uric acid, surrounded with a considerable quantity of very pure ammoniaco-magnesian phosphate: the mass of the stone consisted of mulberry calculus. Another, weighing $15\frac{1}{2}$ ounces, had a nucleus of uric calculus, round which was a mass of ammoniaco-magnesian phosphate, intersected with laminæ of uric acid. It is rare to find calculi composed of four distinct substances. Our author saw one which had four separate layers, or strata, of uric acid, oxalate of lime, ammoniaco-magnesian phosphate, or phosphate of lime. He examined four calculi, the nuclei of which were extraneous substances, viz. a garden pea, a needle, a hazel nut, and part of a bougie. Besides the animal matter, the first calculi contained phosphates only; the second, phosphates and oxalate; and the other two were also destitute of uric acid.

The fourth class of calculi, those found in the urethra, offered no peculiarities to the observer. Indeed, the classification of Mr. Brande does not refer so much to any distinct properties of the substances arranged, as to their position and accidents; their *habitat*, as the botanists express it; for their properties seem to depend merely upon their exposure to the urine in the different stages of its elaboration; nor is there any manifest variety of circumstances affecting their formation, except in so far as they are found in streams of that fluid in its nascent or in its perfect state. The calculi in the urethra, accordingly, are only small calculi which, passing from the bladder, have been detained and lodged in the membranous part of that canal, and, there, have been exposed to the same process of slow accretion, from the passage of urine, which they would otherwise have undergone, from lying in the bladder exposed to the action of the same fluid. One of the calculi of this description, which Mr. Brande examined, was of a singular appearance. It was a perfect sphere, of about half an inch in diameter, coated with

small regular crystals of the ammoniaco-magnesian phosphate in its purest state.

Our author closed his experiments with examining the calculi of different animals. In the horse, sheep, rhinoceros and rabbit, the calculi consisted of the phosphates in part, with animal matter and carbonate of lime. In the dog, no carbonate of lime was found; and, in the hog and ox, no phosphates; the calculus being wholly composed of carbonate of lime and animal matter. No uric acid nor oxalate seems to have been detected in any of the specimens. Some of them were of a large size: a calculus from an old dog weighed sixteen ounces.

From this indication, it is very evident that one conclusion may safely be drawn: that, in most instances, the stone is connected with the formation of uric acid. Sometimes that acid forms calculi in the kidneys, which after creating sufficient torment in that region, pass into the bladder, and become the nuclei of still larger, more painful, and more fatal accretions of other substances; while, in many instances, the same acid continues to augment the mass, either alone or combined with other substances. To prevent its formation, then, becomes a most important object; and both Mr. Brande and Mr. Home propose, for this purpose, the exhibition of some alkaline earth, which may neutralize and carry it off. But, before adverting to this preventive remedy, we may notice the additional proofs adduced, in the papers now under review, to evince the hopelessness of any attempts to solve the stones already formed, at least in the present state of chemical science.

In arguing this point, Mr. Brande (or, as Mr. Home always terms him, Mr. W. Brande) proceeds more by principle; his patron Mr. Home comes forward to his protection with cases. Mr. Brande clearly shows, that an alkali administered to a calculous patient stands no chance of reaching the uric concretion in a caustic state; for the urine contains both phosphoric and carbonic acid uncombined. But experiment clearly shows, that neither carbonates nor subcarbonates exert any sensible action on uric acid: in other words (as we apprehend), the affinity of the uric acid for alkalies is weaker than the affinity of carbonic acid for the same bodies: therefore, alkaline

liquors cannot act as solvents of the uric calculi. On the other hand, if acids be administered with a view of attacking the phosphates, the formation of the uric acid is greatly favoured. This, Mr. Brande's experiments have also taught him. And as for the injection of solvents through the urethra into the bladder, he justly observes, that although we were not ignorant (as we are) of the kind of calculus in each case, the frequent introduction of instruments into the bladder, and the long continuance of the operation, would form insuperable obstacles to this mode of treatment, which, though recommended by great names, has always been speedily relinquished when tried.

Now come Mr. Home's cases: which plainly demonstrate, that where the use of alkaline medicines was supposed to have relieved the patient by dissolving the calculus, on examination no such effect was found to be produced. In two cases, where the violence of the complaint appeared to have subsided, the dissolution was taken for granted, and ascribed to alkaline medicines; but, on dissection, the calculi were found in great size, only imbedded in cysts, from the enlargement of the prostate gland, which often takes place late in life. In some instances, again, the increase of calculous concretions was found to proceed rapidly, while the patients were going on with courses of alkaline medicines. One person took these remedies for four or five years, and, at his death the bladder was found nearly filled with light, spongy calculi of different sizes, no less than 350 in number. Another, who had taken soda, both mild and caustic, for some months, and then submitted to the operation on the symptoms increasing, was found to have a calculus, which was surrounded with a coat of triple phosphate, one tenth of an inch thick, the rest being a mixture of uric acid and phosphates; from whence it is fair to infer, that the alkali increased the formation of triple phosphate, though it checked the production of uric acid. In a third case, of a very virulent nature, the exhibition of alkalies did not even prevent the formation of uric concretions. Mr. Brande's experiments however, in 1808, having rendered it highly probable that, in the great majority of cases, alkaline medicines would operate powerfully in checking the growth of uric

calculi, by neutralizing the uric acid before it could form a concretion, this inquiry has lately been resumed by that industrious and skilful observer, with the aid and advice of Mr. Home; and the result of their joint labours is given in the fourth of the papers examined at the head of this article.

Upon consultation with Mr. Hatchett, they were led to fix upon magnesia, in preference to the other alkaline substances; and the event appears to have justified this very natural and judicious choice. Some preliminary trials showed, that the mild, safe, and easily prepared earth in question, diminished the quantity of uric acid in urine, which contained a larger proportion of it than any of the alkalies, however copiously administered. But this point is of such primary importance, that we must notice the four cases in which a further examination of the matter was prosecuted.

The first was that of a gentleman sixty years old, who, apparently from the habit of drinking acid liquor, had acquired an uncommon tendency to secrete uric acid, and had his urine constantly mixed with that substance, in the form of red sand or crystals. He took, first, the subcarbonate of soda, and then of potash, without any amendment from the former, and with but a slight relief from the latter. His urine being previously examined, he was ordered to take fifteen grains of magnesia three times a day, and in a week the uric concretions diminished sensibly: in three weeks, they were only observed occasionally. The same medicine has been continued for eight months, and no calculi have been voided; nor has there been any material deposit in the urine. He has also been cured of heartburn, and other stomach complaints. Another gentleman, of about forty years of age, who suffered greatly from a similar complaint, and tried the alkalies in vain, has been wholly cured by a six weeks' course of magnesia, in the quantity 'of twenty grains every night and morning,' (we wish Mr. Brande would say more distinctly whether he means *twenty* grains a day, or *forty*), without any change of regimen whatever. The third case, is that of a gentleman forty-three years old, who has, for about a year, succeeded in driving away repeated attacks of the uric acid, by taking magnesia for a few

weeks at a time, when the fit comes on. The fourth case is that of a more confirmed calculous tendency, which has nevertheless yielded, in a great degree, to the free use of magnesia; with this addition, that since the patient began it, he has been free from gout, contrary to his former habits.

The different qualities of magnesia and the alkalies thus observed, led our author to make some experiments on their respective effects upon *healthy* urine. Subcarbonate of potash and soda occasioned a copious precipitation of the phosphates in the urine, during the first two hours after these salts were taken into the stomach; and, after that, no further effect was produced. The same alkalies, saturated with carbonic acid, threw down the phosphates less copiously and rapidly. Magnesia, administered in the same circumstances, produced a much smaller and slower precipitation. Instead of this effect reaching its maximum in a quarter of an hour, as it did in the case of the alkalies, it did not reach that point in less than six hours; and on this circumstance our author concludes, that its value in calculous disorders chiefly depends. Lime, either in the caustic or mild form, produces a very slight effect; and its nauseous taste, as well as difficulty of administering it in sufficient quantities, manifestly preclude its use in this disease. In one case, carbonic acid, exhibited in aerated water, was found to keep the phosphates (which the urine was greatly charged with) in a state of solution; and, when left off, those salts were again voided in their solid shape.

Thus far, then, we think, it may safely be hoped, at least with as little risk of disappointment as usually attends our speculations in medical science, that some light has been thrown on the method of treating a disease, of all others which prey upon the body, the most inimical to the happiness and comfort of man. Where the object is so valuable, it is natural for us to feel uncommonly anxious and distrustful, as well as more than usually eager in our wishes for its accomplishment. Instead, therefore, of being satisfied with what Mr. Brande and Mr. Home have already effected, we would expect them to persevere in their experimental inquiries: multiplying the number of their observations on real cases: and, as it were,

leaving no stone unturned to attain the complete mastery of this great problem. Much will be done for medicine, if they should go no further than perfecting the preventive application of magnesia to the uric calculus. A great number of calculous cases, of the worst description, will be prevented: those, to wit, in which the stone in the bladder is formed on a uric nucleus. Many others of the same class will be greatly relieved. Those, in which composite calculi consist partially of uric acid; and all that class of complaints with which the region of the kidneys is afflicted, by the uric concretions formed there, will be entirely removed. Should such a step in the healing art be made, we need scarcely despair of living to see the phosphates themselves attacked by it, and yielding to some equally simple and safe remedy.

While we are, however, indulging in these visions, and anticipating the final eradication of all calculous disorders, we unluckily cast our eye on the last of the papers which are now before us, and find, that Dr. Woollaston has been discovering a new species of urinary calculus, at the very moment that his learned brethren were occupied in extirpating the already sufficiently numerous specimens of the old catalogue. This new species is extremely rare, our author having only met with it twice. It resembles the triple phosphate more than any other kind; but is more compact, and consists, not of laminæ, but of a confused mass of crystals, having a yellowish lustre and semitransparency; but it is formed of a peculiar substance. Dr. Woollaston made as many experiments on this substance, as the limited quantity of it in his possession would allow. When burnt, it gives a smell quite peculiar to itself. When distilled, it gives a fetid carbonate of ammonia, and an animal oil, leaving a spongy coal. It is readily dissolved by all the pure alkalies and by lime water: it is even solved by the carbonates of potash and soda. The acids dissolve it also, except the citric, tartaric, and acetic. Neither water, alcohol, nor saturated carbonate of ammonia, dissolve this singular substance. From its disposition to unite with both acids and alkalies, Dr. Woollaston suspects it to be an oxide; which is confirmed by its forming carbonic acid in distillation; that is to say, if we take it for granted, as he seems to do, that this acid

does not exist ready formed in the mass. If it does contain oxygen, our author admits, it must be in a quantity insufficient to give it acid properties; for it produces no effect on the color of litmus paper. On the supposition of its being an oxide, and to distinguish it from other bodies of that class, Dr. Woollaston proposes to name it the *cystic oxide*, on account of its having hitherto been only found in the bladder.

This excellent chemist concludes his paper with some curious observations on the connexion between the production of uric acid in birds, and their food. The white matter contained in their urine, and voided along with their dung, was found by M. Vauquelin to consist principally of that acid; and our author examined with some care in what manner its quantity was affected by the diet of those animals. In the dung of a goose fed on grass, it only formed $\frac{1}{200}$ part; in that of a pheasant fed on barley, it amounted to $\frac{1}{14}$; in that of a hen which fed on insects as well as vegetable food, in a barn-yard, it was much more abundant, and mixed with lime. The solid part of the dung of a hawk fed wholly on flesh, was chiefly uric acid; and the evacuation of a gannet, feeding solely on fish, consisted altogether of urine; for the only solid parts were uric acid. 'It seems, (our author concludes) deserving of inquiry, what changes might be produced in the urine of any one animal by such alterations of its diet as its constitution would permit; for, as far as any inference can be drawn from these varieties which naturally occur, it would appear that persons subject to calculi consisting of uric acid, as well as gouty persons in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet; but that the preference usually given to fish above other kinds of animal food, is probably erroneous.'

The mention of Dr. Woollaston's paper naturally leads us to reflect on the important services which this truly philosophical inquirer has formerly rendered to the branch of science now under consideration. It was his important discovery of the nature of gouty concretions, which first ascertained the analogy long suspected between gout and stone; and, besides giving the first analysis of those concretions which had ever

been experimentally attempted, he also first (with the able assistance of Mr. Tennant, a name far too well known to need the humble tribute of our admiration) investigated the composition of the calculi, consisting of neutral salts. The discovery of Scheele, perhaps his most important discovery, brought to light the nature of uric calculi; and, at the same time, made us acquainted with a new acid. The existence of this acid was suspected in gouty concretions; and it was generally imagined that they consisted of the acid alone, or combined with animal matter only. Dr. Woollaston showed by experiment that they are composed of uric acid and soda. Until he carried his inquiries into the other classes of calculi, it was only by conjecture or vague analogies that their constitution was known; but to him we owe as perfect an acquaintance with their component parts, as Scheele had left us of the uric calculus. Having thus completed our analysis of the calculi formerly known, he has, in his present communication, brought to light both a new calculus, and a body. His former paper was published in the Philosophical Transactions for 1797; and the one now before us forms an important appendix to it. It is not within our province to take any further notice of the first tract; but we could not conclude this article without recalling it to the recollection of the scientific reader, and reminding him of his great obligations to Dr. Woollaston.*

Description of an affection on the Tibia induced by Fever, with Observations on the Treatment of this Complaint. By THOMAS WHATELEY, Member of the Royal College of Surgeons in London. 8vo. London. 1810. pp. 59. Colored plate. Callow.

From the Medical and Physical Journal, for December, 1810.

THE complaint which this essay professes to describe, is a disease of the tibia. It is pointed out by one or more small openings in the integuments lying upon the surface of this bone, through which an instrument may be passed into its cavity. A

* See also a valuable collection of historical notices, as well as analytical researches, on urinary concretions, in Dr. George Pearson's paper, Phil. Trans. 1798, Part I.

proper examination with the probe will detect one, or sometimes several loose pieces of bone lying in this cavity; and which being larger than the external opening, are necessarily confined within the tibia, and being dead matter, certainly and invariably prevent the healing of the ulcer.

The curative process is simple and effectual. The external opening must be enlarged to a dimension that admits of the exfoliated portion of bone being removed; and then with easy dressings, a proper attention being given to the general health, and care taken to avoid the causes of irritation, the ulcer soon heals.

The only question seems to be how this opening into the cavity of the tibia is to be enlarged, for the purpose of extricating the portion of dead bone. Most surgeons would use the trephine; Mr. Whateley, however, prefers caustic. (*Kali purum.*)

This disease, which is undoubtedly an internal exfoliation of the tibia, is, perhaps, generally a *sequela* of fever; or, is some way connected with, or excited by, a preceding febrile affection.

"Having observed," says Mr. Whateley, "that in almost all the cases of this kind, which I have had in hand, the complaint had been preceded by fever, I have been led to consider the disease in question as a febrile affection of the part. I should inform the reader, that the case, now before him, is not one which frequently occurs. I do not recollect having, in the course of my practice, had more than about thirty of them intrusted to me. But having, in the course of many years, observed a certain peculiarity in the disease, I took notes of most of the cases, which came under my notice, conceiving that I might, at some future time be thereby enabled to afford useful information to the public, on the treatment of this complaint.

"These cases occur more frequently at the middle period of life, than at any other.* In most instances, that came within my observation, the preceding fever was of considerable duration; and sometimes so violent, during its continuance, as to

* See next page for note.

confine the patient to his bed, and often to be attended with delirium. Some time after the patient's recovery, he was seized with pain in one of his legs. The period at which this pain commences, is not the same in every case. In some patients, it takes place immediately on the termination of the fever; in others, it is not experienced till a few days afterwards; in others again, not till several weeks, and in others, not till many months have elapsed. Yet I should observe, that I have always found the disease to make its attack, within twelve months from the termination of the fever."

The phenomena and symptoms of the complaint are described with minuteness and precision.

"In most of the cases, the pain in the leg was acute, and appeared to the patient to proceed from the interior of the Tibia. The pain, indeed, was not equally violent in every case; as some of the patients were confined to their bed for several days, or weeks, and deprived of sleep for a considerable time; while others were able to go about. No external swelling or inflammation is, in general, to be expected, immediately on the commencement of pain. Yet these appearances are seldom long in following; they are usually visible within a week, and sometimes in two or three days. In some cases, a general inflammation and swelling over the whole limb first appeared, and soon after a more circumscribed inflammation, near the affected part of the Tibia, in which a fluctuation of pus might be felt. In other cases, the inflammation and swelling never extended to the entire limb, but were, with the subsequent formation of pus, confined to a small circle on the Tibia. In

* I took notes of twenty-two cases which have been under my own care, the ages of these are as follow:

Of the age of 14 - -	1
——— 18 - -	1
——— 19 - -	1
Between 20 and 30 - -	7
——— 30 and 40 - -	11
——— 40 and 50 - -	1
	22

most instances, the suppuration was confined to one part of the tibia; and this was often about the middle of the leg, and the centre of the bone. In many cases, however, the suppuration took place in other parts of this bone: namely, within a few inches of the instep, a litttle below the knee, or in some of the intermediate spaces. Sometimes it has happened, that two or more distinct suppurations, and as many separate openings into the cavity of the bone have taken place. These were sometimes found within an inch or two of each other; at other times further apart. There was some variation likewise, as to the time of their appearance: some weeks elapsing between the commencement of each suppuration, in some cases, while in others, all appeared nearly at the same time.

"Although a fluctuation of pus might be discovered, within a short time after the disease had taken place, the matter did not usually burst through the skin as hastily as it does in some other suppurations. This discharge, however, sometimes took place within a week or two, from the first appearance of matter; but in many cases, the pus continued in a confined state, for many months, and required the use of the lancet to set it free.*

"In most of the cases, the violence of the pain ceased: the inflammation and swelling abated, as the fluctuation of matter was felt; and in a short time afterwards, the patient was generally able to walk about. Soon after the discharge of matter, the wound usually contracted to so small a size, as scarcely to admit the point of a probe; and a small quantity of pus continued to ooze from the aperture. In some cases, however, the ulcers closed up for a few days, and then burst out again. If in this state of the disease, a probe is passed carefully into the little orifice of the ulcer, it usually enters readily, into the cavity of the tibia,† where the loose piece or pieces of exfo-

* In the greater number of cases, the suppuration is small, often containing not more than a tea-spoonful or two of pus. In other cases, however, I have seen a larger suppuration, in which, an ounce or more, of matter is formed.

† In one or two cases, I have perceived this orifice so small, as not to admit the round end of a probe, of the usual size. In a few other cases, it has been so oblique, as to be entered with much difficulty by that instrument.

liated bone are frequently to be felt. Under these circumstances, I have never known a perfect cure to take place, unless where the appropriate treatment had been resorted to.

"In this disease, the surface of the tibia, immediately around the orifice, becomes often, though not always, knotty and irregular; and the periosteum is sometimes thickened, so as to give an appearance, resembling that of a venereal node. This disease, however, differs totally from those affections of the tibia produced by syphilis, as it does likewise from those usually denominated necrosis, or from any of those which are produced by scrofula. In short, as far as I have been able to observe, it is a disease sui generis, the relic of fever, usually affecting the tibia.*

"From the violence of the pain, and sudden erosion of the bone, an inflammation appears to attack its internal part. Suppuration takes place; and matter is first formed there, and then makes its way through the substance of the bone, to the outer integuments. As, however, the ulcerative process in the bone sometimes takes place in different parts, thereby producing several apertures in the tibia, matter is, in these cases probably, formed in distinct places, previous to the ulcerations in the bone. One of the most remarkable circumstances attending these cases is, that one or more pieces of loose bone are usually found within the cavity of the tibia, opposite to each aperture.

"Some of these exfoliations are so small and thin, as to require a nice examination with the probe, in order to discover them. They are, however, generally found of a larger size, sometimes even exceeding an inch in length. I have always found these exfoliations of an oblong and spiral shape, and evidently separated from the internal laminæ of the tibia within the cavity of the bone. The separation of such a portion of bone is probably the effect of the previous inflammation and ulceration; by which it is deprived of circulation, and of course

* I have some recollection of having, many years ago, seen this disease in the fibula; but in that case, the tibia was likewise affected.

loses its connexion with the living bone, of which it was once a part."

Having inquired into the probable cause of this disease, and passed through the various appearances it assumes, the author proceeds to describe the curative process.

"During the painful and inflammatory state of the disease, and before the erosion of the bone has taken place, it would be proper to apply emollient fomentations and poultices to the limb: to keep the patient in bed; and to endeavour to ease the pain, by occasional doses of opium. I must confess, however, that I have seldom seen the disease in this stage. I have commonly been applied to, after the erosion of the bone has taken place, and when the patient has been, in some measure, able to follow his usual employment. In this stage of the disease the cure of the ulcer, by the common external remedies, is impossible: the loose piece or pieces of bone, pent up within the cavity of the bone, must set that mode of procedure at defiance. The imprisoned exfoliations, therefore, must be extracted; and this is desirable to be done, by the mildest methods possible, avoiding all unnecessary pain and operations to the patient.

"The loose bone within the cancelli, is commonly longer than the ulcerated aperture in the bone. It is in many cases, indeed, considerably longer, and though confined to its situation, it may sometimes be taken hold of by a pair of forceps, and moved upwards and downwards, in its bony case. The examination by the probe should, therefore, be made with great care and gentleness; otherwise, much unnecessary pain will be excited. It may happen also, that the exfoliated bone may be removed from the favourable situation it is in, when it is directly under the ulcerated aperture, and be pushed under the arch of the tibia, as I have more than once experienced; by which the difficulty of extracting it must be greatly increased.

"As the orifice in the tibia, as well as the exfoliations, are of different sizes in different cases; and as they are generally covered by the integuments and granulations, it requires a nice examination with the probe, to ascertain the true state of the parts. In some cases, as before observed, the aperture in the tibia is so small, as not to admit the round end of the probe, to

pass into the cavity of the bone, and the contrary end, or an eyed probe, will pass into it with so much difficulty, that no accurate examination of the parts beneath can be made. In most cases, however, it freely admits the round point of a probe; and our attention should first be directed, to find out the size of the aperture in the bone. Having ascertained this, we should next examine, whether there be a loose piece of bone within the cancelli. The size of the aperture may be known, by passing the probe, in a gentle manner, from side to side in all directions. But it is not easy either to find out the exfoliated piece, or to ascertain its exact dimensions, when discovered.

"Where the aperture in the tibia is large, and the exfoliated piece very small, the latter may be moved about, with the end of the probe, so readily, as to leave no doubt of its easy extraction, by the introduction of a pair of fine forceps. Under such circumstances, the operation should be instantly performed; and if the whole be removed, the wound will generally heal up in the course of a few weeks: the application of any common dressing, aided by bandage, will be sufficient for the purpose. The utility, therefore, of first examining with great nicety, by the probe, in order to avoid, if possible, the use of more violent remedies, must be evident.

"In more difficult cases, the exfoliated piece may, likewise, be readily moved in the cavity of the bone, by the end of the probe; yet the hand of the operator will be sensible that it is confined, by the sides of the surrounding tibia. In some cases of this kind, the opening in the tibia may be large enough to admit of our taking hold of the exfoliation, by a pair of common forceps; but in others, it is so small, that a pair of the finest forceps that can be made, cannot be made to enter, so as to embrace the bone. When possible, we should endeavour to extract the exfoliated bone by the forceps; and in these attempts I have readily succeeded, when the exfoliation was not longer than the aperture in the tibia. But where it proved to be of greater length, which might be known, by its being moved upwards and downwards in its bony case, by the forceps, but whence it was not possible to extract it; I have been sometimes able to accomplish the extraction, by moving it

upwards or downwards, as far as it would go, and then raising one end of it. Where I could not succeed by this procedure, I have been sometimes obliged to resort to a more violent method; that of endeavouring to break the exfoliation into two parts, by means of the forceps. This I have been able to do, where it has not been very strong, and having thereby extracted it, the cure has been speedily completed.

"There are other cases, however, in which methods very different from these, must be taken. We may have the strongest reasons for concluding, that there is a loose exfoliation, within the cancelli, and yet may not be able to feel it by the probe. In this case, as well as when the opening in the tibia is too small to admit an exfoliation to pass, by any methods above described, we must apply the kali purum to the integuments around the ulcerated opening in such a manner, and in such quantity, as to destroy them, to the extent of about half an inch from the centre of the opening. This is done with a view to expose as much of the surface of the tibia, as is requisite for the cure.* The proceeding, here recommended, is attended of course with some pain; and it requires particular attention, in order to prevent the kali from destroying more of the integuments than is necessary; and from penetrating through the orifice into the cancelli.

"This caustic may be applied in several different ways. That which I prefer is, to take about as much of it (bruised into parts of the size of a pin's head) as will lie upon a seven shilling piece, and apply it, both to the ulcerated opening in the skin, and to the surrounding integuments, to the extent already mentioned. Pieces of dry lint, or adhesive plaster, should be applied around the caustic, to prevent it from extending on the adjoining skin. In a few minutes, the kali liquefies, and begins to operate.

"The leg should be placed in a horizontal position; and if the caustic appear to act equally, the limb must remain in that position for about ten minutes, or a quarter of an hour; after

* The orifice of the bone is situate so near to an angle of the tibia, in some cases, that it does not admit of its being exposed, to the extent of half an inch, on all sides of it.

which, a pledget of dry lint should be applied over the kali, large enough to cover the adjoining sound skin. The leg should then be slightly bandaged with a flannel roller.

"It is to be observed here, however, that during the action of the caustic, some additional attentions are not unfrequently required, in order to ascertain the depth to which it has penetrated. If in one part, the integument is thicker than in another, or if the caustic is acting more powerfully on one part than on another, a little of it may be removed with the probe, to or from such part; or a little more fresh caustic may be applied to any particular part, if it appear to require it. During the action of the caustic, particular attention must be paid to its effect on the little ulcer on the skin, and on the orifice in the tibia, as it is desirable to prevent its penetrating the cancelli, much harm and disturbance being likely to arise from its action on this part.

"If the integuments adjoining to the wound be thick, the kali may be applied to this part, nearly in the same quantity as to the other parts; as, in general, it ceases to act before it can penetrate into the cancelli. But if the integument, on this part be very thin; or if there be no integument or granulation over the ulcerated orifice, a small piece of lint should be passed down it to the cancelli, previous to the application of the kali, to prevent it penetrating too far: a piece about the size of a pea will generally be sufficient. This should be rolled with the finger and thumb, and firmly pressed down the orifice by a probe, till it passes into the cancelli, where it should remain. If this should not perfectly plug it up, a second, or even a third pledget should be applied. This is often attended with some pain, from the pressure made on the granulated flesh at the bottom of the wound, which generally springs from within the cancelli, and is exquisitely sensible. The kali should remain on the part to which it is applied, for about six hours. After this, the wound should be dressed twice a day with some dry lint, and an emollient poultice. In about a week or ten days, from the application of the caustic, the slough will come away.

"If the caustic have performed its office, the surface of the tibia will be perfectly bare; and now the unequal, knotty state

of the bone, and thickening of the periosteum, will often be seen; and if the granulations on the surface of the orifice have been destroyed by it, the loose exfoliation within the tibia will often be visible to the naked eye. But where the granulations are not destroyed, the internal exfoliation cannot be discovered without introducing the probe into the wound, as before directed.

"Sometimes the surface of the tibia around the ulcerated aperture, will be found, on the separation of the slough, not to be sufficiently exposed. In this case, a little of the kali must be applied a second time, to such parts as require it. This should be done in the course of a few days, otherwise the wound will begin to fill up with granulations. Though a repetition of the caustic is seldom attended with as much pain as accompanies the first application, it being seldom necessary to destroy a fresh portion of the skin; it should, nevertheless, be applied with all the precautions, recommended in the first instance; the like dangers being to be guarded against, on the subsequent application as on the first. It must be remembered, however, that a proper quantity should be applied, otherwise the surface of the bone will not be sufficiently exposed, when the new slough separates. When the second application has been properly conducted, we seldom have occasion for a third.

"The surface of the tibia, with the aperture into its cavity, being thus exposed to view, we proceed to search for the loose exfoliation, if it be not already visible. The orifice in the tibia must again be carefully examined by a probe, and as this examination can now be assisted with the eye, the size and situation of the exfoliated piece will for the most part be readily discovered. If indeed the granulations within the cavity of the bone shall not have been destroyed by the caustic, some impediment to the examination may arise from them, as they are in general extremely sensible, and are apt to bleed, on the slightest touch of the probe. But as the exfoliated piece generally lies buried in these granulations, and is perfectly detached from the contiguous bone, it may frequently be discovered, yielding to the pressure of the probe.

"Its situation being discovered, and its size, compared

with that of the aperture in the tibia, pretty fairly ascertained, we must endeavour to extract it by the forceps; and sometimes we shall succeed, where the endeavours made, previous to the application of the caustic, have failed. The exfoliated bone, however, will often be found to be larger than the opening in the tibia, in which case it will, in general, be impossible to extract it, till the orifice has been widened.

"If the trephine be used for this purpose, it may be sometimes difficult, though probably never impracticable, to remove the piece incircled by the instrument after it has been worked to a proper depth. I remember to have succeeded many years ago by this method, in extracting a large piece of bone, pent up within the cancelli. The case to which I allude occurred to me when I was a very young practitioner, and has been already published. From the date of this operation, there has not occurred a single case in my practice, in which I have found it necessary to use the trephine. I have, for some years, practised with great success a method of enlarging the opening in the tibia, which is much more simple, as well as less painful. It is by means of the kali purum; and the way in which I use it is as follows: I apply this caustic to the bare surface of the tibia, around the hole, and to the sides of the hole itself, after the separation of the slough; taking care to guard against its touching the adjoining parts, or penetrating into the cavity of the tibia: each of which dangers may be prevented, by the application of the lint, as before directed. In about ten minutes after the application of the kali, the bone should be covered with a pledget of lint; and on the following day the kali should be again applied to all the parts, as before. After this the limb should be dressed twice every day with a fresh poultice; it should be lightly covered with a flannel roller, and the patient may be generally permitted to take moderate exercise. In a month or less from this period, an exfoliation of all that part of the bone, to which the caustic has been applied, will take place.* This ef-

* I have not known this exfoliation take place in less than three weeks from the application of the caustic; and it happens, not unfrequently, that it is not separated in less time than a month. The granulated flesh around it, therefore, generally covers this dead part of the bone before it is taken out

fect may be known by occasionally pressing a probe upon the surface of the bone. For when the intended exfoliation is accomplished, the exfoliated piece gives way, on the pressure of the instrument. I do not, however, suffer it to be removed immediately on discovering its separation from the body of the tibia; but generally let it remain in its situation two or three days, in order to give sufficient time for its being completely disengaged, that it may be removed with less danger of being broken. I then complete the removal in the most gentle manner; sometimes by the forceps, sometimes by passing either the one side of a pair of forceps, or a director, under it, as circumstances may dictate.* Having proceeded thus far, my attention is next directed to the removal of the loose pieces of bone within the cancelli. In almost every instance in which the process above described has been followed, I have found this bone at perfect liberty. But before I attempt its extraction, I always introduce a probe, in order to ascertain its exact position. In most of the cases which have come under my care, it is readily felt by the probe; but the practitioner should be prepared to meet with some disappointment here. There are cases, in which, from its smallness, it is not only extremely difficult to find the imprisoned piece, but even to discover any vestige of it, on the first examination. In three cases I found it so small as to require a very nice management of the probe to feel it; in two of these cases, the loose piece was found immediately after the enlargement of the aperture in the tibia was accomplished; in the third case, it was not found till two days afterwards. Out of the whole number of cases, which have occurred in my practice, I can advert only to one in which I have searched in vain for it: nor do I feel warranted to conclude, that even in this case

This, of course, obscures it from the view, but when loose, it is as readily extracted, with the loss only of a few drops of blood, as if it was perfectly exposed.

* This exfoliation generally exhibits a very curious appearance. From an examination of seven or eight pieces of bone thus exfoliated, from as many different cases, it is evident that the ulcerative process, at the commencement of this disease, erodes the substance of the tibia, in this part, to such a degree, as to render the enlargement of the hole, by the caustic, more certain, than it would have been, had the tibia been of its usual thickness.

there was no such piece; but rather suppose, that, by its minuteness, it either eluded the search with the probe, or escaped unperceived in the dressings. This solitary instance occurred, before I was aware how very small some of these pieces are found to be. In the three cases I have mentioned, the loose pieces found would have escaped my notice, had I relaxed but a little of that perseverance with which I searched; and any conclusion against their existence, drawn from the disappointment, would, of course, have been erroneous.

"After having enlarged the aperture in the tibia, by the caustic process already described, and having found the loose exfoliation, I have in almost every case been able to extract it. There has, however, been an instance or two, in which it has been found still too large to pass through the aperture; although it could be moved upwards and downwards, in its bony case. In these cases I have been obliged to adopt the method already described in page 11; viz. of breaking the loose bone into two parts, in order to liberate it from its confinement. Had I not been able to do this, I must probably have been obliged to make use of the trephine.

"Having carefully examined all the pieces thus extracted in the different cases which have come under my observation, I have invariably found them to be exfoliations of the interior laminæ of the tibia; no marks of affinity with the external surface of the bone having been found on any of them. I am likewise certain that none of the exfoliated pieces extracted from within the cancelli were any part of the exfoliation made by the caustic for the purpose of enlarging the hole in the tibia. The greater number of them were ascertained to be within the cavity of the bone, before any caustic was applied to the tibia. Besides which, I should inform the reader, that the pieces exfoliated by the caustic were removed with so much care, that no fracture of them was likely to occur; and if it had, it must have been perceived.

"After I have extracted one piece of bone from within the cancelli, I examine the bottom of the wound with a probe, to ascertain whether there be any more pieces: this I continue to do occasionally for about a fortnight, knowing that no cure can

be obtained if any portion of bone be left behind. The hole indeed would fill up with granulations, but a small oozing wound would remain. When the cancelli are cleared of these exfoliations, the wound should be dressed in the most superficial manner, and with the most simple dressings; applying compresses and a roller lightly over them. In the greater number of these cases, the skin is perfectly cicatrized in the course of six weeks, or two months, from the removal of the exfoliated pieces; and the inequality on the surface of the tibia is generally removed. The scar, indeed, and the external appearance of the leg, are somewhat different, after the cure in these cases, from what I have observed in most others. In a few instances, however, I have seen the wound continue open for several months, after the removal of the bones. In one case, indeed, it continued open nearly two years; during which time a probe could be passed within the cancelli, to the extent of four or five inches; but no material inconvenience accrued from it to the patient during this interval; and it afterwards healed up."

Under this treatment, the usual termination of the disease, with Mr. Whateley, has been in a speedy recovery. There have, however, occurred sometimes severe symptoms, which have not only endangered the limb, but have even destroyed the patient.

In two cases in middle aged women, in the intervals of child-bearing, the *infortunia* were most formidable, and the consequences extremely serious.* They are attributed by the author, to a violent use of the forceps in extracting the exfoliations

* These were violent inflammation, attended with great pain, extending itself through almost the whole of the tibia; and the formation of pus on the surface of its different sides, in almost every direction, as well as within the cavity of the bone. In one of these cases, the symptoms arose to such a height, as to destroy the patient in the course of a few weeks. In the other, the patient survived; but the irritation and inflammation were so great, for the first six months, as at length to leave the tibia eroded through its entire thickness, a little below the knee; where, on moving the limb, a kind of joint was observable. By the use of opium, fomentations, and emollient poultices, (the limb being kept in an horizontal position) the disease gradually abated; and after some large exfoliations from the substance of the tibia had been thrown off, the use of the limb was restored.

from the cavity of the *tibia;* but they are more fairly referrible to the patient's walking considerable distances while under surgical treatment, and serve as a proper warning to those who may have the management of similar cases; and will induce them to confine their patients to the house, if not to the bed, during the treatment.

The remaining part of the essay consists of twenty-two cases, with a plate and explanations.

The value of this pamphlet arises from its practical utility, and as affording additional information from that best source, an observance of nature, on a disease, though not of frequent occurrence, of great importance. If we differ in some points, possibly of opinion only, with Mr. Whateley, we admit in the fullest manner the service this species of medical literature is to the science; where, even if the hypothesis be wrong, the facts, if correctly observed and faithfully related, afford materials for a more legitimate theory, and more certain conclusions.

"This disease," says Mr. Whateley, "differs totally from those affections of the tibia produced by syphilis, as it does likewise from those usually denominated NECROSIS." There certainly are no facts that can induce us to suppose it has a syphilitic origin; but that it is a species or variety of the disease examined and so well described by Mr. Russel, (*Practical Essay on a certain Disease of the Bones, termed Necrosis*, 8vo. Edin. 1794), and by professor Weidman, (*De Necrosi Ossium, fol. Francfurti ed. Mænum*, 1793), there is surely but little doubt. The disease, which these writers have described in the advanced period, Mr. Whateley has seen in the incipient state; or, possibly, in a mild variety, where only a small portion of bone has perished, but which being confined within a bony sheath, has kept up ulceration, until it has been removed. There are some particulars, however, in which the disease described by Mr. Whateley differs from the definition of *Necrosis*, as given by Mr. Russell. In Mr. Whateley's cases there seems to have been simply an exfoliation of the internal substance of the tibia, which, falling down among the cancelli and lying loose, keeps up an irritation and purulent discharge; while in the cases that

Mr. Russell denominates *Necrosis*, a covering of new osseous matter surrounds the dead bone. The period of attack is likewise different. Mr. Russell found *Necrosis* to be properly a disease of the early period of life. "I have never known," he says, (p. 92) "a case of it in which the attack began after the twentieth year, except in cases of *Necrosis* of the lower jaw. About the age of puberty, or from twelve to eighteen years of age, is the time of life at which patients are most liable to be attacked." Mr. Whateley found that in twenty-two cases, eighteen occurred to persons past thirty. We are disposed to think, from some cases that have fallen under our own particular knowledge, as well as from an examination of the numerous *sequestra* preserved in an anatomical museum of great extent, that Mr. Russell has too much contracted the period in which this disease attacks; and that its depredations in advanced life are not confined to the lower jaw. Professor Weidman gives a plate (*Tibula tertia*) that so much resembles the plate, and so closely accords with the description given by Mr. Whateley, that the identity of the diseases seems very completely established.

The mode of treatment here adopted for enlarging the opening in the tibia, through which to extract the *sequestra*, does not quite agree with the dexterity of modern surgery: neither does it appear to us adequate to the object. We assert this, subject however to the correction which the great experience of Mr. Whateley can well supply.

A Commentary on the Treatment of Rupture, particularly in a state of Strangulation. By E. GEOGHEGAN. 8vo. pp. 95. London.

From the Medical and Physical Journal for 1810.

To deviate from the common path of the profession with safety and success, is not the work of ordinary minds; and he may justly be considered as the benefactor to his art who shall suggest any real improvement in the treatment of a disease which has occupied the attention and the pen of so many eminent writers. The interesting little work before us has, besides its intrinsic merit, a claim to our notice, in as much as

the subject was many years since opened in the pages of this Journal. In the year 1800, the author published some observations on this disease, in our fourth volume; and his farther experience has confirmed him in the validity of the opinions he then advanced. The present Commentary on the treatment of Hernia begins with a detail of the general directions given by different authors to accomplish the reduction by the hand; and after citing the opinions of Mr. Pott, Dr. Munro, Mr. Bell, Mr. Cooper, and Mr. Lawrence, the author says, (p. 28)—

"When I consider the talents, and the extensive opportunities which these writers possessed, it is not without anxiety that I contest their opinions; but experience has abundantly shown that they are ineffectual in practice; and I feel a conviction that they are unsupported by theory. I have given their own words that their full meaning may the more clearly appear, and that my inference may the more readily be understood.

"The first objects for our consideration are the structure of the parts concerned, the phenomena which the disease exhibits, and the pathology which may be fairly induced. When hernia has taken place in the groin, it has passed through two apertures: that which is next the abdomen is formed by the tendon of the transversalis and internal oblique muscles, and a fascia that lines the transversalis; the external aperture is formed by a separation of the tendinous fibres of the external oblique muscles; they are filled up with the spermatic chord in males, and by the round ligaments of the uterus in females; they form an oblique canal, which in old ruptures becomes straight, or nearly so. In crural hernia, it passes under Poupart's ligament, through a small aperture, in a tendon formed for the transmission of the femoral vessels. During a length of time it slips in and out of the abdomen uninterruptedly: a circumstance to which I would particularly direct the attention. Suddenly, after any great exertion of the body, such as lifting a weight, coughing, sneezing, &c. or external injury, it refuses to return into the abdomen. On examination it appears tumified, tense, and inflamed, accompanied with excessive pain, and derangement of the functions of the viscera, and universal dis-

turbance, the inflammation rapidly increases, the distention is prevented by the rigidity of the tendinous aperture, or by the neck of the sac which contains the intestine—hence strangulation."

The author then goes on to state, what, however, has been stated by others; but the consequences which he deduces are his own, and important; that the strangulation and its symptoms are not produced by any altered state of the abdominal aperture, and that the inflammation and enlargement of the intestines are the essential points to be considered.

He then observes,

"From what has been stated, it appears that the indication of cure generally laid down, is not warranted by the nature of the disease, nor of the treatment, namely, to return the hernia through the apertures by pressure with the hands; because, as mentioned before, it is not the situation of the intestine, it is the enlargement of it, which occasions the symptoms; and that state should, in the first instance, forbid the attempt on account of its obvious impracticability."

These remarks lead the author to, and prepare the reader for, a very material alteration in the treatment of strangulated hernia in its first stages, which we shall state in his own words.

"With the view of affording a satisfactory explanation of the practice, which I think ought to be pursued, founded on the principles which I have endeavoured to establish, I shall describe my own manner of treating a case, and the order which I think ought to be followed, and which has succeeded under my arrangement in, I believe, more than 36 cases since the year 1796.

"I place the patient in a recumbent position, with his shoulders a little raised to relax the trunk, but the pelvis not raised, as that would put the fasciæ on the stretch; the knees are to be drawn up. If the parts have not been irritated by handling them, or the body disturbed by jolting it about, or by any such roughness, I proceed directly to apply cloths, wet with cold water, expose the entire body to the open air, the doors and windows

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being open. This practice usually succeeds within an hour.* If it does not, I surround the hernia with my hand, or hands, at about its middle, in the way that I would grasp a gum elastic bottle, to press out its air or other contents, by gently approximating its sides, always holding in view that the tumor is to be emptied, and not pushed up,† and that a little assistance to the compressing force, which the coats of the intestine, (reacting from distention) are exerting, by lessening its area, even in a small degree, the air will be strongly impelled against the part of the tube which is closed, and through which it is only necessary that it should obtain exit, to effect our purpose. When it is small, as in femoral hernia, or in protrusions, which do not pass below the groin, or are elsewhere situated, it may be done with the fingers and thumb of one hand. Having applied the hands, I do not remove them for fifteen or twenty minutes, aware that reiterated impulses irritate, and that the effects of compression are lost each time that it is intermitted.

"Cases which have been long strangulated, and those which are accompanied with great pain and tension, are exceptions to this practice, so far as the employment of the hand, on the principle that the reaction of the intestine is at a pitch sufficient to propel the air through the coalesced tube, if its passage is possible, and that handling parts in such a state would endanger mortification or rupture of them. My chief reliance in such is on bleeding, proportioning the quantity by nearly the following scale:

"If the patient is young and robust, I take off sixteen ounces; if old, or weakened by previous illness, from six to ten ounces, should even debility‡ take place if attended with

* In some cases where I could not immediately attend, I have directed that cold applications should be used until my arrival, and after an hour, they informed me that they were seized with a shivering; that they heard the wind rush out of the hernia; and that they were instantly relieved.

† I never press the hernia in any direction, or at all towards the aperture.

‡ It is very material in this, indeed in every disease, to discriminate between that debility which is the effect of pain, anxiety, and restlessness, and that which is the effect of impaired health or of original constitution. In the former, bleeding is advisable; in the latter it is generally improper to any extent.

great pain, I repeat the bleeding after three hours; but if the pain yields, I omit it; all this time I apply nothing topically, except cold water, and strictly avoid all attempts with the hand.

"I direct no medicine internally, and by these means I have succeeded in the instances before mentioned; and I am persuaded that they will scarcely ever fail, provided, that the taxis in the usual manner had not been practised, and that such other exasperating causes, as putting the patient with his head to the ground and his heels up, and jolting him about, had been omitted.

"Strangulated hernia, under this treatment, will in many cases continue several days without serious mischief; and in the event of the operation being required, peritoneal inflammation will be provided against.

"The tobacco clyster is the next remedy which I would advise; in a robust subject in the proportion of one drachm to a pint of water, which I would repeat of double that strength, if the first produced no effect; in an infeebled person, half a drachm should be first tried.

"In the event of failure in our efforts to remove the strangulation, the operation of enlarging the apertures by the knife must be had recourse to; the precise time at which we should lay aside all other means, and commence this practice, is not easily decided on.

"In a young and full person suffering great distress, I would not defer it longer than six or eight hours, particularly if the hernia had been much handled, and also if bleeding had not been used; but where no mischief had been produced by external causes, and this evacuation had been premised, it might be deferred sixteen or twenty-four hours, always watching if the belly became sore to the touch, or much inflated, in which case to operate instantly. In an aged or debilitated subject it may be delayed longer. I have known it to continue in such, six, seven, or eight days, and afterwards to be removed without an operation."

Such is the treatment, and such the means which the author recommends in the strangulated hernia previous to operation. The points on which he differs from others are, chiefly, abstaining from violent attempts to reduce the gut, and objecting to the use of purgative medicines. He is the advocate of bleeding, of the application of cold, and, in short, of every antiphlogistic plan to reduce the size and inflammation of the intestine preparatory to its return. On the whole, we consider this commentary as a valuable addition to our information on a disease of great importance, both from its frequent occurrence, and the imminent danger which accompanies it; and we therefore strongly recommend the work to our readers.

ORIGINAL PAPERS.

On the Hydatids of the Uterus.

By T. C. JAMES, M. D.

The following case having some time since fallen under my notice, I shall, without any apology, venture to submit it to the press, with a few observations necessarily resulting from it.

Having been called to visit a patient who was supposed to be in the fifth month of her pregnancy, but who for several weeks previously had been distressed with uterine hemorrhage, at first in a moderate degree, but gradually increasing in violence until it became truly alarming. Upon my first seeing her, I was convinced from the severity of the pains which had come on some hours before, with regular intervals of ease, that abortion would inevitably take place, and thought it most safe to ascertain her precise situation, by an examination; by which I found the os uteri dilated to about the size of a half dollar, and a substance presenting very similar, as regarded the touch, to the placenta. Presuming that this was attached immediately over the os uteri, I thought it best to separate or perforate it with my finger, expecting that the liquor amnii and fœtus would then be immediately expelled: but, to my surprise, instead of these, there was discharged a mass something resembling a small placenta verging towards putrefaction with half a common wash-hand bason full of transparent spherical vesicles, from the size of a pea up to that of a small walnut, full of water: after which the hemorrhage ceased. I could not detect any appearance of the impregnated ovum or fœtus. The vesicles were connected with each other, and to the surface of the more solid mass resembling the placenta, by short pedicles or necks; they were extremely delicate, and easily broke down upon being handled. The woman recovered happily and speedily.

Mr. Home, in the second volume of the Medical and Chirurgical Transactions, relates the case of a young woman who died

in what was supposed the third month of pregnancy, where the ovum was filled with hydatids nearly of the same kind I have described; where, although the situation of the placenta was readily discovered, there was no appearance of fœtus or funis. This ingenious physiologist does not think it a change in the structure of the placenta, but a general affection of the amnion. When this disease takes place, as he observes, the natural healthy actions for the support of the fœtus are so much impeded that its growth is arrested; and when the patient does not early miscarry, the fœtus disappears; and in all the instances where miscarriage has taken place in a more advanced stage of the disease, no fœtus, as far as is known, has been found.

A case very similar in every respect is also related in the 2d volume of the London Medical and Physical Journal. The quantity of hydatids in this instance was measured, and found to be three pints and four ounces. In Mauriceau's and in Smellie's collections* are also related corresponding histories of cases of uterine hydatids; and Morgagni, in his elaborate work De causis et sedibus morborum, after referring to a number of authors for similar cases, relates his having found numerous hydatids in the uterus of a bitch that died suddenly during a supposed pregnancy.†

A question might here occur, perhaps not readily to be solved, whether the hydatids are to be considered as the *cause* or *effect* of this singular morbid state of the uterus: whether they originate from the death of the fœtus and the destruction of the ovum, which I should rather presume to be generally the case; or whether, as in the liver of sheep and even that of the human species, they arise without any known or ascertained cause, and are to be considered as a peculiar original disease of the viscus itself. That this may sometimes be the

* Mauriceau Observ. ccclxxvii. Smellie. Pt. II. No. 3.

† Ruysch in the 1st vol. of his valuable works has given two very curious and accurate plates of these hydatids of the placenta or uterus although he appears to be uncertain with respect to their nature; for at first he seems to have supposed them to be minute glands of the placenta degenerated into hydatids: and afterwards considers hydatids generally to be the extremities of sanguiferous vessels which have changed their original nature and degenerated into a morbid state. Ruysch Observat. Anatom. et Adversar. Anat.

case would appear from some facts related by Vigarous, a professor of Montpelier, who has of late years written on the subject, and who asserts that he had under his care the case of a woman who never obeyed the calls of nature, without discharging from the uterus more or less hydatids of the size of a large horse bean.

May we not suppose that many of the cases recorded by medical writers, under the title of dropsy of the uterus, and which are so frequently to be met with in nosological works, were in fact cases of hydatids of the uterus?

For a long time, it appears, that it was not determined from whence these hydatids had their origin, nor how they could be formed in such vast abundance in animals, until Hartmann in 1685 discovered them to be alive and to possess a peculiar structure and power of motion; and after him Dr. Tyson, an English physician, in a paper published in the 17th vol. of the Philosophical Transactions, proved very satisfactorily that they were a peculiar species of worm, which he termed *Lumbricus Hydropicus*. His investigations and experiments were on hydatids discovered in dissecting a gazella or antelope; and they appear, from his description, to be precisely the same as are frequently found in sheep and described by Dr. John Hunter and other writers. Tyson gives a plate of the worm of its natural size and magnified. His discovery excited the attention of naturalists; and Pallas, Hunter and others, having prosecuted the subject with considerable industry, confirmed the fact of their vitality.

In the course of their inquiries, they discovered in this worm new characters, which induced them to arrange it under the genus *tænia;* and it is by Pallas called *tænia hydatigena.* Linnæus's term is *hydrahydatula.*

Notwithstanding this, writers have differed with respect to the vitality of these singular vesicles; and Tyson, although he acknowledged their vitality in the antelope, seems to doubt of the animation of those in the human subject that had fallen under his notice. But Pallas has attempted to do away this difficulty, by supposing that in the progress of their growth, and as their appendices, or necks, delineated by Tyson, are con-

tracted, and they assume more of the dilated and globular form, that they proportionally lose their vitality, till they are ultimately entirely deprived of both the one and the other on which the character of the *animal* depends; and they become mere inert and lifeless vesicles, though still frequently filled with a transparent or limpid fluid.

There appears, according to Dr. John Hunter, to be this difference between human hydatids and those found in sheep: that the former are found in great numbers in the same sac and of various sizes but have no neck or mouth, although he afterwards acknowledges that the mouth is not essential to the hydatids of sheep: and a late *Italian author*,* who has written very fully on worms infesting the living human body, relates the case of a patient dying suddenly with symptoms of apoplexy, where, on examining the brain soon after death, a congeries of vesicular hydatids were found in each of the lateral ventricles, which he compares to small grapes suspended by a stalk. Each of these vesicles, upon being opened, was found to contain a worm with a spherical body full of water, and with a projecting neck and head not dissimilar to a species of tænia. He has given a curious plate of this worm. In this case the disease must have been beyond the reach of medicine and death inevitable. Lieutaud also mentions two cases of hydatids being discovered after death in the meninges of the brain. Hist. Anatom. Med. vol. 2. p. 145. And the celebrated Ruysch asserts that he had met with a considerable number of hydatids in the plexus choroides. Ruysch Adversar. Anatom. p. 8.

I should have mentioned, that the very accurate Dr. Baillie, in his Morbid Anatomy, considers the hydatids found in the brains of sheep as exactly similar to those of the human liver, and inclines to the belief that the generality of the hydatids found in the human body, even those of the liver and of the uterus and placenta, are a sort of imperfect animals; and although there may be some difference between them in simplicity of organization, that this need be no considerable objection to the opinion, as life may be conceived to be attached to the most simple form of organization.

* Valeriano Luigi Brera.

Dr. Hunter, above referred to, who has written a very ingenious paper on this subject, in the first volume of the Transactions of a Society for the Improvement of medical and chirurgical Knowledge, remarks, that of the numerous cases of hydatids related by writers, hardly any proved fatal whenever they found an outlet; so that whenever their existence is suspected, it must be of great consequence to endeavour if possible to procure them an exit or discharge. But when this cannot be effected, perhaps mercury used with some freedom promises to be as useful as any internal medicine that can be exhibited; and one case is related in a medical publication (Medical Transact. of College of Physicians of London, vol. 2.) where it appears to have produced a favourable effect exhibited in the form of calomel.

In this instance 135 hydatids were brought up from the lungs by coughing in the course of three or four months. They were of different sizes, from that of a pea to that of a pullet's egg. Cases similar in some degree are related both by Bonetus and Morgagni, Lieutaud and Baillie. May we not with Dr. Baker suppose, that in the above case the hydatids might have originated in the liver, and from thence, by the effects of disease found their way to some of the branches of the trachea.

It has been observed in rural affairs, that the hydatids that infest sheep are either prevented or destroyed by changing their pasturage from low and humid situations to a pure and dry atmosphere, on the sides of hills and mountains: and perhaps in the human subject, when there is a disposition to generate these vesicles, a similar change of air and situation may be attended with good effects, together with a generous and nutritious diet, easy of digestion, combined with a proper degree of invigorating exercise, and in fact a plan calculated to restore and support the healthy action of the vascular system generally, and that of the absorbents in particular.*

* To prevent the regeneration or new formation of hydatids or vesicles, Burns recommends introducing a bougie into the uterus or frequently injecting some astringent fluid into that viscus.

Case of Singular Disease of the Uterus.

By Dr. D. J. SWINNEY.

On the 27th day of March last, I was called upon to attend a female patient labouring under an excessive flow of the catamenia. This had continued for four days with scarcely any diminution: the discharge was also attended with a considerable quantity of whitish fluid approximating to the nature of half-formed pus.

On inquiry, I found a disease of the uterine system had existed to a troublesome degree, for about four months; and that for two weeks past only, the patient had been prevented from pursuing her usual employment as a servant in the family. I afterwards ascertained, that she had occasionally complained of it for more than ten years. She would sometimes tell her fellow servants that she felt something move in her body like a living creature. This only excited their ridicule without contributing to her relief.

When I saw her first she was very much debilitated from her menorrhagia, and felt excruciating pain in the region of the uterus. These symptoms were accompanied with severe retchings and vomiting, and an expectoration of tough phlegm; also with fever, dyspnœa, and want of appetite: headach, giddiness and pain in the loins; the extremities frequently very cold: these were attended with palpitations of the heart and faintings. Afterwards, by examination, I ascertained that there was a large abdominal tumor, which presented a soft feel to the fingers. This tumor was situated just below the umbilicus. Most of the symptoms were subdued by the use of the following remedies.

After some of the more common medicines in cases of vomiting had failed, and I had found that nothing remained in the stomach, I directed a large blister to be laid over the stomach, which very speedily checked the vomiting. By the use of rubigo ferri and the external application of flannels, kept constantly wet with cold vinegar, to the region of the uterus, the menorrhagia was subdued on the fourth day from the time I was called in. The debility was

in some degree removed by the use of a Peruvian bark bandage applied round the body. At this time also the fever entirely disappeared. The patient now lay much easier and seemed to inspire her husband and attendants with great hopes of her recovery; but on the fifth day the uterine region was again attacked with pains resembling labour-pains, which continued for two days, baffling the power of anodyne medicines. The patient from this time became restless; her jaws set; and her stomach so irritable that it would not retain any thing.

From existing symptoms on the sixth day, I concluded that a cancer or scirrhus uteri must be present. On the seventh, the pain subsided; the patient became speechless; and no relief could be procured but by injections and external applications. The patient appeared beyond the reach of the medical art. Thus she continued almost insensible until the ninth day, when she expired with very little pain.

Permission being obtained I opened the body, in the presence of a young gentleman,* and found that the uterus was very much enlarged: that the cervix uteri formed a scirrhous tumor: that the body of the uterus contained at least three pints of yellowish purulent matter though not well formed; and that the os tincæ afforded too small a passage for its free evacuation. I found also an adhesion between the anterior surface of the cervix uteri and the parts adjacent; this was easily separated by the fingers.

On examining the texture of the uterus it appeared no more than half the thickness of a healthy one and of a harder, firmer, membranous feel. Posteriorly there was discovered a small cavity in the uterus divided from a much larger one by a longitudinal septum: this septum was of a placental feel and appearance; and was attached both to the cervix and fundus. Within this cavity was contained about two gills of pure well-formed pus. In this situation and totally saturated with this fluid I found a mass of hair intervolved together forming a ball about the shape and size of a common hen's egg. This hair was of various lengths, from the eighth of an inch to that of nine inches and a half; and its colors were those of a light

* Mr. Charles Busti.

brown and reddish sandy, and all shades between them as nearly as I can judge. This hairy mass is now in my possession.

There was also a circular surface of about three inches diameter in the fundus uteri, that was covered over with irregular scaly ossifications nearly close to each other, about the thickness of a common goose quill, some very small and others nearly half as large as the nail of the little finger. Their convexity was towards the internal surface of the uterus. The other contents of the abdomen showed not the least appearance of disease.

The woman when she died was about sixty years of age; had never had more than one child and that about her 37th year; had none by her last husband to whom she had been married about 14 years, in which time she had been once or twice attacked with symptoms of abortion and was delivered of an imperfect fœtus. She was accustomed to hard labour as a servant in the kitchen of a large family, and was given to ebriety for several of the last years of her life.

I close this detail by submitting these facts to the gentlemen of the faculty for their candid consideration and investigation; for I am desirous that the cause of such a strange phenomenon may if possible be ascertained.

D. J. SWINNEY.

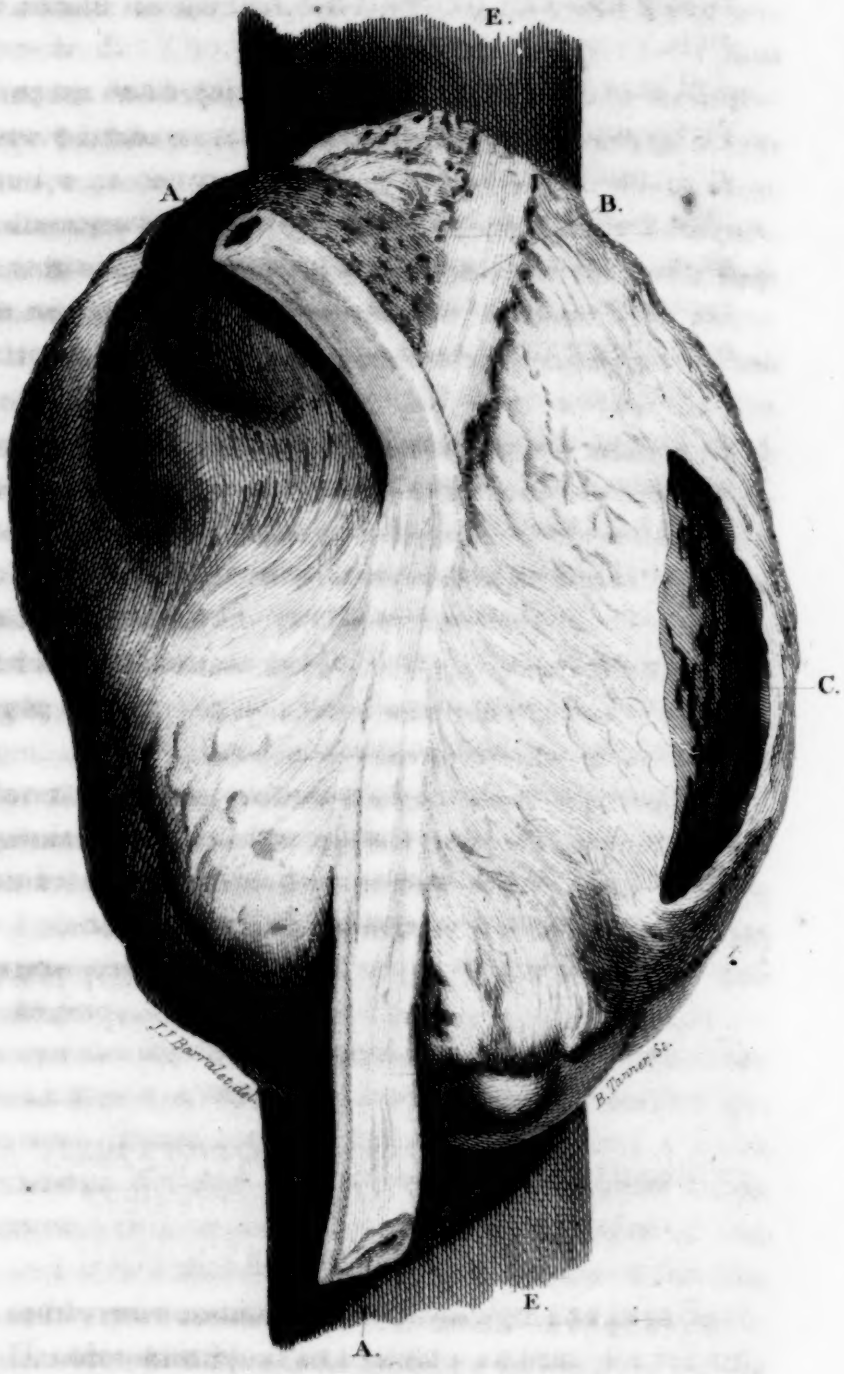
Blockley, May 15th, 1811.

An Account of a Case of Aneurism of the Femoral Artery, which was spontaneously cured.

By JOSEPH PARRISH, M. D.

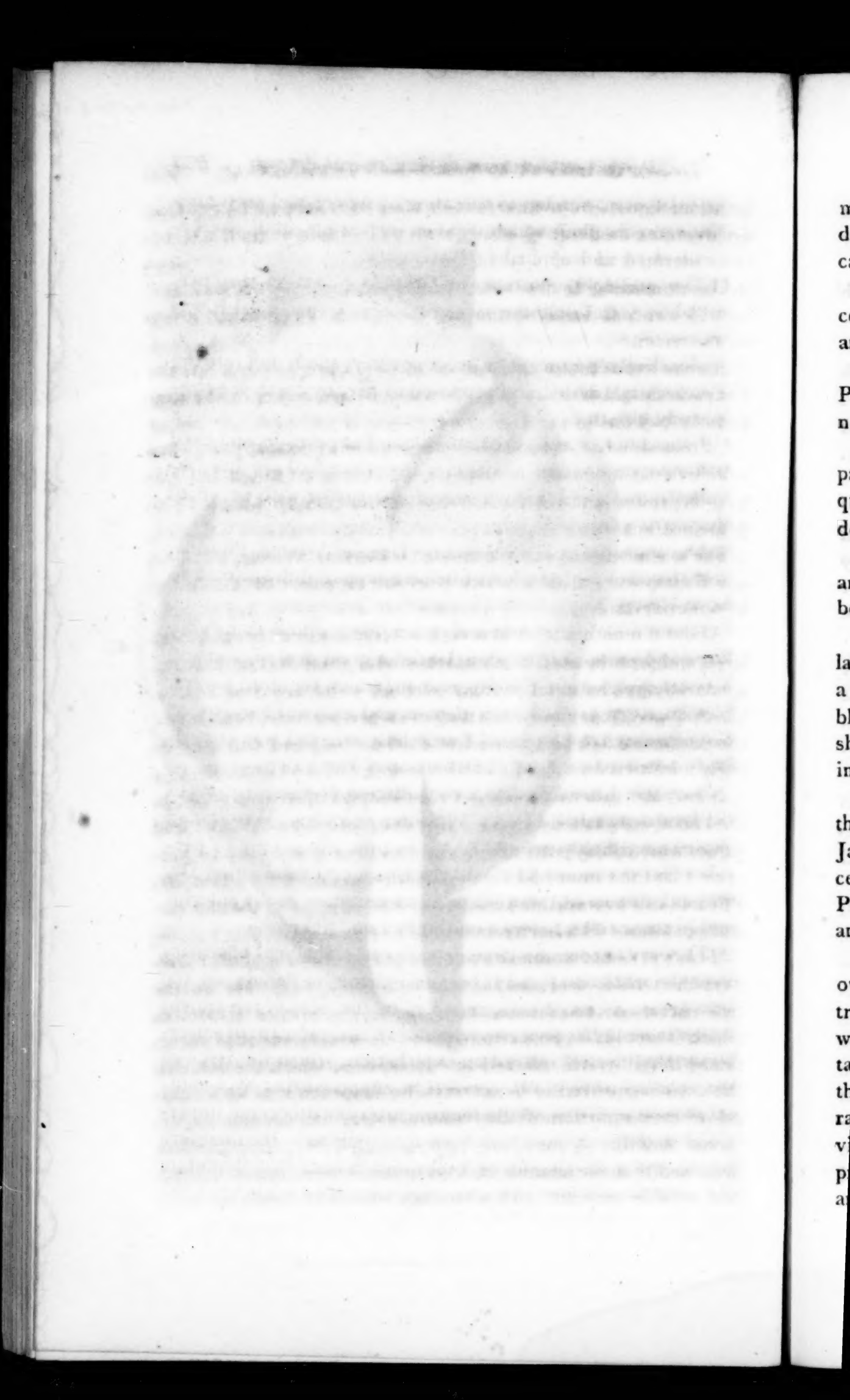
ELIZABETH VANDINE, aged about forty-three years, came under my care as a patient of the Philadelphia Dispensary, on the 14th of third month (March) 1810. She was suffering considerable pain from a tumor, situated on the inside of the left thigh, just above the knee, and extending along the course of the femoral artery, for several inches.

The patient informed me, that a few weeks before when carrying a bucket of water, she felt a sensation in the above



ANEURISM of the FEMORAL ARTERY.

Published by Anth.y Finley, July. 1811.



mentioned part, similar to the sting of a bee; and soon after discovered a swelling which went on increasing, until she became alarmed and applied for assistance.

On examining the tumor, an obscure pulsation was perceptible; and from all the circumstances, I concluded it was an aneurism.

As she lived in a garret, in great poverty, admission into the Pennsylvania Hospital was proposed to her, to which she strenuously objected.

I directed her to keep the limb as quiet as possible, and used palliative treatment to allay pain. I continued to see her frequently, and marked the increase of the tumor with no small degree of anxiety.

She was visited at my request by Doctors Wistar, Physick, and Hartshorne; all of whom I believe supposed the disease to be aneurismal.

About a month after first seeing her, the tumor became very large and prominent. She experienced a great deal of pain and a smart sympathetic fever supervened, which required several bleedings. At last tired out with pain, and aware of her danger, she consented to be removed to the hospital, and was accordingly admitted on the 2d of fifth month (May) 1810.

Soon after her admission a consultation of the surgeons of the house was called by Dr. Physick; and Drs. Wistar and James accordingly attended. At this time it was easy to perceive that the tumor had diminished; and on examination Dr. Physick discovered that there was no pulsation in the anterior and posterior tibial arteries on the foot and ankle.

This very uncommon circumstance cast a shade of obscurity over the whole case; and it became difficult to decide on the true nature of the disease, and also on the plan of treatment which it would be proper to pursue. In that moment of uncertainty Dr. Physick offered an explanation, which the issue of the case has proved to be correct: he supposed that by a natural process a portion of the femoral artery had become impervious; and thus a cure had been accomplished. He therefore proposed that she should be kept quiet in bed; that moderate and equable pressure with a bandage should be made over the

whole limb; and that a long splint extending from the axilla to the foot should be so secured as to keep the limb and body fixed.

She continued in the hospital until the 11th of seventh month (July), when being desirous of returning home she was discharged.

After this I saw her but seldom until the 9th of fourth month (April) 1811, when she applied again to me at the dispensary. At this time she had regained the perfect use of her limb; but was complaining of a "beating in her breast," as she termed it. On examination I perceived a strong pulsation on both sides of the thorax; and suspecting her disease to be an aneurism of some of the great vessels contiguous to the heart, I commenced a plan of palliative treatment, which was pursued by one of my colleagues, Dr. Samuel Stewart, who attended in my place for a week preceding. On the morning of the 18th of the same month she died suddenly.

The husband positively refused the liberty of opening the body; but I prevailed on him to allow me to examine the thigh: and in the presence of Drs. Stewart and Elmer I dissected out the tumor, which is the subject of the accompanying engraving.

The aneurismal sac, at its posterior part, was attached to the os femoris; and the artery, as appears by the plate, was situated on the anterior part: the sac was filled with coagulated lymph of a dark chocolate color.

A little reflection on the situation of the sac and artery will give a ready explanation of the mode adopted by nature in the cure. The sac was attached to the os femoris: hence in the enlargement of the tumor, as it could not pass in a posterior direction, it was necessarily projected forward, and became, as was stated, very prominent: now the result of this was, that the artery above and below the sac, being retained in its usual situation by its natural investments, the portion of vessel connected with the sac was (as the latter became prominent) placed so completely on the stretch, that its two internal surfaces were brought into contact. Adhesive inflammation took place; and

for the space of four inches the cavity of this important vessel was rendered impervious.

Cases of the natural cure of aneurism are certainly rare; but I apprehend, they cannot with propriety be denied. A remarkable cure, which occurred in the carotid artery, may be seen in the "Transaction of a Society for the Improvement of medical and chirurgical Knowledge, vol. i, page 119. It is contained in a paper intitled, "Uncommon appearances of disease in the blood vessels, by M. Baillie, M. D. The carotid artery was the vessel involved in the disease; and a plate accompanies it.

In vol. ii. same work, page 268, there is another account of spontaneous recovery from aneurism, by John Major Wilson, house surgeon to the Westminster Hospital.

In our own country we have lately been presented with a case of natural cure, communicated by Dr. Lyman Spalding of Portsmouth, N. H. to Dr. Samuel Mitchell of New York, and published in the Medical Repository of that city. Hexade III. vol. i. No 4. page 348.

Some other detached cases of spontaneous cure of this formidable disease are to be met with in books less accessible, than those already quoted, but surely not of higher authority.

Now it is presumable that these facts are not merely interesting as matters of curiosity, but they go far to establish some important practical inferences.

Several modes of treating aneurism have been adopted by surgeons. Pressure, so continued as to bring the sides of the artery into contact, has been recommended. Scarpa admits the correctness of the practice of compression, when made above the aneurismal tumor and on a sound part of the artery: he forbids the use of it in cases of painful aneurism, also in those "attended with steatomatous ulcerated, earthy disease of the arterial coats." In cases favourable for compression, he asserts that bandages have produced radical cures of aneurism, and should not be entirely disused.

Other surgeons have used compression with a pad and tourniquet, and suddenly stopped the circulation. In the tumor this treatment is then continued until the limb becomes œde-

matous, when the tourniquet is directed to be removed; and the pressure of a pad and roller is said to be afterwards sufficient. The pain induced by sudden and powerful pressure is so great, that few patients can be found who will submit to it; and hence has arisen an objection to the practice.

Laying open the aneurismal sac and securing the artery, above and below it, by ligature has also been tried; but the danger of the vessel being in a diseased state, at the parts where the ligatures are to be applied, has formed a solid argument against this mode of treatment.

John Hunter, with his usual sagacity, perceived these difficulties: and, in order to remove them, he tied the artery on a sound part, above the sac, in a case of popliteal aneurism: adhesive inflammation was thus induced in the sides of the vessel; and its cavity was rendered impervious.

But even this great improvement has been found liable to objections: thus, for example, the sudden check to the circulation, through the limb, has in some few instances been so complete that, before the anastomosing branches could dilate sufficiently to carry the requisite supply of blood, mortification has taken place: and instances have occurred wherein the adhesive process did not go on; and, after the ligature separated, a dangerous hemorrhage arose from the femoral artery.

To meet the various difficulties and yet to fulfil the same intention, in cases of aneurism in any part of the femoral artery, long continued compression has been employed. Thus for example in the London Medical Review, for July 1810, under the head of medical and surgical intelligence, it may be seen that at Paris, in the Hospice de l' Ecole de Médicine, M. Dubois has employed compression on the thigh for popliteal aneurism, and has continued it even so long as 15 or 18 months. In the present account we are not informed of the exact place where the pressure was made, nor whether it was commenced in a very gradual manner.

The superficial situation of the artery in the groin renders it an eligible spot for the application of pressure; and, to insure success, it should be commenced in the most gradual manner. Thus if something could be contrived like the spring truss and

pad, with a screw adapted to it, half a turn might be made with the screw every day, until the requisite degree of compression was effected. In this manner it would be so gradual in its operation, that patients would be likely to bear it without difficulty; and it offers another very important advantage: as thereby the anastomosing vessels would have time gradually to enlarge, and thus carry on thecircul ation through the limb. For the latter plan I am indebted to Dr. Physick; who informs me that he intends to make trial of it in the first fair case that shall come under his care.

INTELLIGENCE,

MEDICAL AND PHILOSOPHICAL.

CURE OF HYDROPHOBIA.

Extract from a Danish Gazette called "The Day," published at Copenhagen, for the month of October 1810.

The counsellor of medicine, Dr. Schallern, of Bajreuth, in Germany, has lately, at Carolinereuth, on the estates of the grand hunter, Fr. Hardenberg, by the use of *Belladonna* and *Cherry Laurel water*, perfectly cured a girl four years old, who had been bitten by a mad dog. The disease was completely formed, the patient being averse to liquids.

It is thus proved, that there exists a remedy for this dreadful disease, even when it has already made its appearance.

Interesting Fact respecting the Digitalis Purpurea.

From the London Medical and Physical Journal, for 1808.

A fact, stated by Dr. Baildon in the Edinburgh Medical Journal, July 1807, may assist in explaining from whence has arisen the diversity of opinion on the action of the *Digitalis*. This gentleman observed in his own case, and repeated the experiment a great many times, that after the digitalis had taken effect, his pulse was not lessened in frequency when he stood erect; it was then upwards of a hundred. When he sat down, it fell considerably; when lying on his back, it fell much more. Thus, during the time it was at forty when lying, it was about seventy five when sitting, and above a hundred when standing. Doctor Baildon found the pulse to vary in this manner in all the patients to whom he gave the medicine in any extent. This is a fact of the most interesting practical nature in the medical history of digitalis; and calls for that minute attention on the part of the faculty, that shall either establish it as a principle in the exhibition of this powerful remedy, or reject it as one of those accidental effects that arise from inexplicable causes.

Glocestershire Vaccine Association, for promoting Cowpox and discouraging Smallpox Inoculation.

THIS Association was formed in April, 1810. At present it consists of sixty-three medical practitioners of this county; all of whom, actuated by a high sense of public and professional duty, have individually and collectively declared, "*That they, considering their knowledge of Cow-Pox, do not believe themselves intitled either to practise or to sanction the use of Small-Pox Inoculation, and that henceforth they renounce it accordingly.*"

A general meeting was held at Glocester, on the 6th of this month; at which it was resolved, that every attempt should be made to draw similar declarations from the medical practitioners in the different parts of the kingdom, because such unequivocal testimonies would most effectually tend to give confidence to the community, and to dispel those prejudices which are still artfully and ignorantly opposed to the progress of vaccination. To promote these objects, the following address was proposed, adopted unanimously, and ordered to be circulated as widely as possible.

TO THE PUBLIC.

We, the members of the GLOCESTERSHIRE VACCINE ASSOCIATION, having for the first time assembled together, take the earliest opportunity of declaring to the public the motives of our conduct. The facts which have induced us to adopt this measure, we trust will be received with attention by every one: they have carried conviction to our minds, and we anxiously hope that they may not be without effect upon the minds of others.

More than *twelve years* have elapsed since our illustrious countryman first made known the results of his accurate and successful inquiries into the powers of vaccination. His experiments have been repeated on the grandest scale; and every corner of the globe has borne testimony to their truth and their importance. By the means which he discovered, the most

pestilential and mortal distemper that ever attacked mankind, has already been banished from many parts of the world; and we believe that, by vigilance and attention, it may soon be eradicated from all. Every year's experience adds strength to this conviction; and we glory in it, as the highest claim that our art can exhibit to the gratitude and respect of our species.

In associating together to diffuse the blessings of a discovery which has already accomplished so much, we are not actuated by any fears respecting its ultimate and universal success. But we feel, that we should neither perform our duty as good citizens, nor as members of a profession to whom the care of the health of the community is intrusted, were we to remain inactive, while we see the murderous and disgraceful practice of smallpox inoculation prevailing around to an alarming extent.

Many of the causes which at present support an ill-founded opposition to the practice of vaccination, must soon pass away; and the next generation will probably wonder at the obstinacy which has been evinced by so large a proportion of our countrymen, in rejecting its great and well authenticated benefits, even after they had been gratefully acknowledged in almost every spot on the habitable world. This perverse spirit we sincerely lament: it is peculiarly discreditable to this country; and we deprecate it as most pernicious and unjust. Smallpox, in any shape, is a dangerous and loathsome disease. It is less fatal in the inoculation than in the natural state; but even this advantage has been purchased at a dreadful expense; thousands have been sacrificed, that a few might be saved.

It is our belief, that every argument for the continuance of this practice, has been removed. Upon this principle, we have associated together; and upon this principle we declare, that no man who has satisfied himself of the preventive powers of vaccination, *can be justified, either in practising or in sanctioning the use of smallpox inoculation.* To those, who, disbelieving in the efficacy of cowpox, have resisted the strongest testimony that ever supported any medical question, we have nothing to say. We are sorry that, on a subject of this kind, any individuals do not feel as we earnestly wish that all should

feel. But we can neither impeach their integrity, nor reprobate their inconsistency. To those, however, who, knowing that they possess an antidote to the ravages of smallpox, and yet become accessary to the propagation of that distemper, another language ought to be spoken. Our meeting this day, and the whole of our previous proceedings, are meant to indicate our *abhorrence* of such conduct, because we consider it as the destructive triumph of avarice or timidity, over professional integrity and independence. This most criminal indecision, has, in our opinion, prolonged the existence of prejudices which might long ago have disappeared. Medical men, who know, and have admitted, the advantages of vaccination, are at least bound to act in conformity with that knowledge. A conscientious and explicit avowal of their sentiments, supported and inforced by a manly and consistent line of conduct, must inspire confidence into the public mind, and facilitate the advancement of a cause, for the success of which every good man must be interested. We have attempted to do so; and we most fervently hope, that our endeavours may not be in vain.

But while we thus express our opinions of the duties which ought to bind us, and the rest of our brethren, we must at the same time remark, that our best exertions will be fruitless, unless the public feeling upon these points in some measure corresponds with our own. While medical men are held responsible for their professional acts, it is despotism to attempt to awe them into practice which their reason and their conscience condemns. We feel ourselves called upon to resist such influence; and we know, that, by so doing, our claim to public confidence and esteem should not be diminished. If we are required to follow the honest decisions of our judgment, in the ordinary transactions of our profession, much more is it necessary for us to do so, in a case which involves consequences like the present.

Did those who employ smallpox inoculation expose their own children only to the dangers of that disease, we might justly bewail their fatal ignorance and obstinacy; but when, in addition to this, they involve a whole district in the same perils, they are *guilty* of *cruelty* and *injustice*, and are answerable for evils, the extent of which no human being can calculate. We dare not,

with our present knowledge of cowpox, become actors in this work of death; and we feel the greatest concern, that the question respecting the dissemination of a plague, should lie at the mercy of any individual.

Were the evidence in favour of cowpox infinitely less perfect than it is, the circumstances which we have stated will always claim the most serious consideration from every benevolent mind: and when they shall have been as accurately and extensively examined as they should be, we confidently expect a determination in unison with our wishes. Should our attempts to accelerate this event prove unsuccessful, we shall at least enjoy the satisfaction of believing that we have not neglected the means which have been placed within our reach, of conferring a substantial benefit upon our race; and we know that they are at this instant greater, than ever were possessed by the members of our profession at any former period.

In conformity with the views just stated, it was further resolved, at the meeting above referred to, that measures should be taken to request the cooperation of the members of the medical profession throughout the kingdom, for the purpose of checking the unlimited dissemination of smallpox contagion; that a central vaccine institution be established at Glocester, as soon as sufficient funds are collected; and that subscriptions be opened at the banking-houses of Glocester and Cheltenham.

The members of this association, in presenting this abstract of their proceedings to the public, earnestly beg for that assistance and support which they think themselves intitled to expect. They confidently appeal to every individual, of every denomination, who either has experienced or wishes to experience the blessings of vaccination, cordially to unite with them in forwarding this great cause. Considerable expense has been already incurred, and the purposes of the association cannot be carried on without incurring more. It is hoped, therefore, that aid in proportion to their importance will be freely bestowed and that the inhabitants of a country, which has given to the world the most valuable of human inventions, will not be backward in contributing to diffuse its advantages.

Glocester, July 25, 1810.

EXTRAORDINARY EFFECTS OF THE EFFLUVIA FROM QUICKSILVER.

Extract of a letter from Lisbon, dated 12th May 1810, giving an account of a singular effect of the fumes of Quicksilver, in the crews of two ships on that station.

A LARGE quantity of quicksilver was saved from the wreck of a Spanish ship about Cadiz, by two or three of our ships, and was placed in their spirit rooms. An alarming illness broke out among the crews, all of whom were more or less salivated. The surgeons, pursers, and three petty officers, who were nearest the place where it was stowed, felt its effects the most, their heads and tongues having swelled to the most alarming degree. We have been under the necessity of sending the Triumph to Gibraltar to be cleared, and to have the people sent to the hospital, this ship having been most affected, having had the greatest quantity of quicksilver on board. The Phipps schooner has been cleared in this harbour, by boring a hole in her bottom, to allow the quicksilver to run out. Every rat, mouse, and cock-roach on board, has been destroyed, and the symptoms of general salivation have appeared in a strong degree. It may be a question whether this effect of the quicksilver was owing to its having lain long in sea-water, or whether the rotting of the leather bags which contained it might have a share in producing these *effluvia*. It is in favour of the latter opinion, that the officers above mentioned, who slept over the bags were most affected, whereas those who slept near the quicksilver, which run lose, were not affected. This was particularly observable in the Phipps schooner, where the men slept on the cables, close to where the quicksilver had flowed in consequence of escaping from the bags, and yet they were but little affected in comparison of the others.

The editors are also informed, from undoubted authority, that three surgeons of the fleet were directed to meet, and after investigating this matter, to report upon it, and that their report confirms the particulars of this statement.

College of Physicians.

At a stated meeting of the College of Physicians of Philadelphia, held July 2, 1811, the following officers were elected for the ensuing year:

President,.....Dr. Adam Khun,
Vice-President,....Dr. Samuel Duffield,
Censors,.....Dr. Thomas Parke,
Dr. William Currie,
Dr. Samuel P. Griffitts,
Dr. Caspar Wistar,
Treasurer,.....Dr. Thomas C. James,
Secretary,.....Dr. Thomas T. Hewson.

University of Pennsylvania.

At a Commencement in the University of Pennsylvania, held the 26th April, 1811, the degree of Doctor of Medicine was conferred on the following gentlemen:

Samuel Ayers, of *New Hampshire.*

John Y. Lansing, A. H. Stevens, Samuel Shuts, of *New York.*

William S. Coxe, William Elmer, of *New Jersey.*

Adam Hays, Jonathan Murdock, Samuel Nixon, John Reynolds, George Schott, of *Pennsylvania.*

Joel Lewis, Allen M'Lane, of *Delaware.*

John Austin, John H. Briscoe, William Hammond, John D. Perkins, William Purnell, Robert J. H. Purnell, J. D. Readel, James Stewart, Matthew Vanlear, of *Maryland.*

William S. Alexander, T. B. Anderson, G. H. Batte, R. L. Bohannen, John Briscoe, William Buchanan, Henry Chambers, M. Clarke, Hawes N. Coleman, Abner Crumpe, Ashley Davis, Charles Edwards, William French, Joel W. Flood, T. Goode, P. E. Graves, John Gilliam, F. J. Mittaner, James Marye, Samuel Meny, John Roanes, J. M. Sheppard, P. B. Tindall, J. Trent, J. Whitlow, A. L. Wooldridge, of *Virginia.*

Benjamin Ballard, Thomas A. Cabarrus, Stephen Davis, of *North Carolina.*

H. C. Glover, A. Garden, H. M. Holmes, Joseph F. Lees, A. De Leon, William M'Caw, N. Starke, F. Y. Porcher, of *South Carolina.*

William Hughes, Joseph M'Murtrie, of *Kentucky.*
Edward Grant, of *Tennessee.*
John Kee, of *Mississippi Territory.*
Robert Abbott, of the island of *St. Croix.*

And on May 30th the degree of Doctor of Medicine was conferred on Thomas C. James, bachelor of medicine and professor of midwifery in the University of Pennsylvania; and on Elias W. Napier, of Kentucky.

Pennsylvania Hospital.

At a meeting of the managers, held at the Hospital May 13, 1811, the following contributors were chosen

PHYSICIANS.

Thomas Parke,
Benjamin Rush,
Philip Sing Physick,
Benjamin S. Barton,
John S. Dorsey,
Joseph Hartshorne, and
Thomas C. James, was elected physician to the lying-in department; and
Samuel Calhoun, physician to the out-patients.

Humane Society.

MANAGERS OF THE HUMANE SOCIETY OF PHILADELPHIA.

Elected March 1811.

Joseph Crukshank, *President,*
Isaac Snowden, *Secretary.*
William Leedom, Matthew L. Bevan, } *Inspectors.*
Dr. Samuel P. Griffitts, Thomas C. James, Joseph Parrish, } *Committee of Correspondence.*
Charles Marshall,
Benjamin Thaw,
Samuel Pancoast, jun.
John Bacon,
Joseph Hornor, *Treasurer.*

Thermometrical Observations.

State of the weather at Philadelphia, for the first six months of 1811.

JANUARY.

Thermometer.....Lowest, - - - - - - 20
Highest, - - - - - - 53
Mean, - - - - - - - 30

Winds.....NE and NW, generally.

This month, on the whole, moderate weather; there being only occasional obstructions to the navigation from the ice, yet changeable and unpleasant. Not much snow: no sleighing.

FEBRUARY.

Thermometer.....Lowest, - - - - - - 13
Highest, - - - - - - 48
Mean, - - - - - - - 30

Winds.....Northerly.

Weather moderate; though changeable. No severe weather till 19th, then real winter weather till near the end of the month.

MARCH.

Thermometer.....Lowest, - - - - - - 28
Highest, - - - - - - 69
Mean, - - - - - - - 40

Winds.....Chiefly easterly.

The month was remarkably mild: scarcely any stormy weather.

APRIL.

Thermometer.....Lowest, - - - - - - 35
Highest, - - - - - - 70
Mean, - - - - - - - 48

Winds.....Mostly easterly.

Moderate, but changeable: not remarkable for heat or cold.

MAY.

Thermometer.....Lowest, - - - - - - 46
Highest, - - - - - - 76
Mean, - - - - - - - 60

Winds.....Variable. More westerly and southerly.

A changeable month: cool for the season. Spring more forward than usual; probably owing to the dry warm weather in March.

JUNE.

Thermometer.....Lowest, - - - - - - 62
Highest, - - - - - - 86
Mean, - - - - - - - 70

Winds.....More westerly and southerly.

Some warm weather in this month, and some unusually cold: on the whole fine weather.

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